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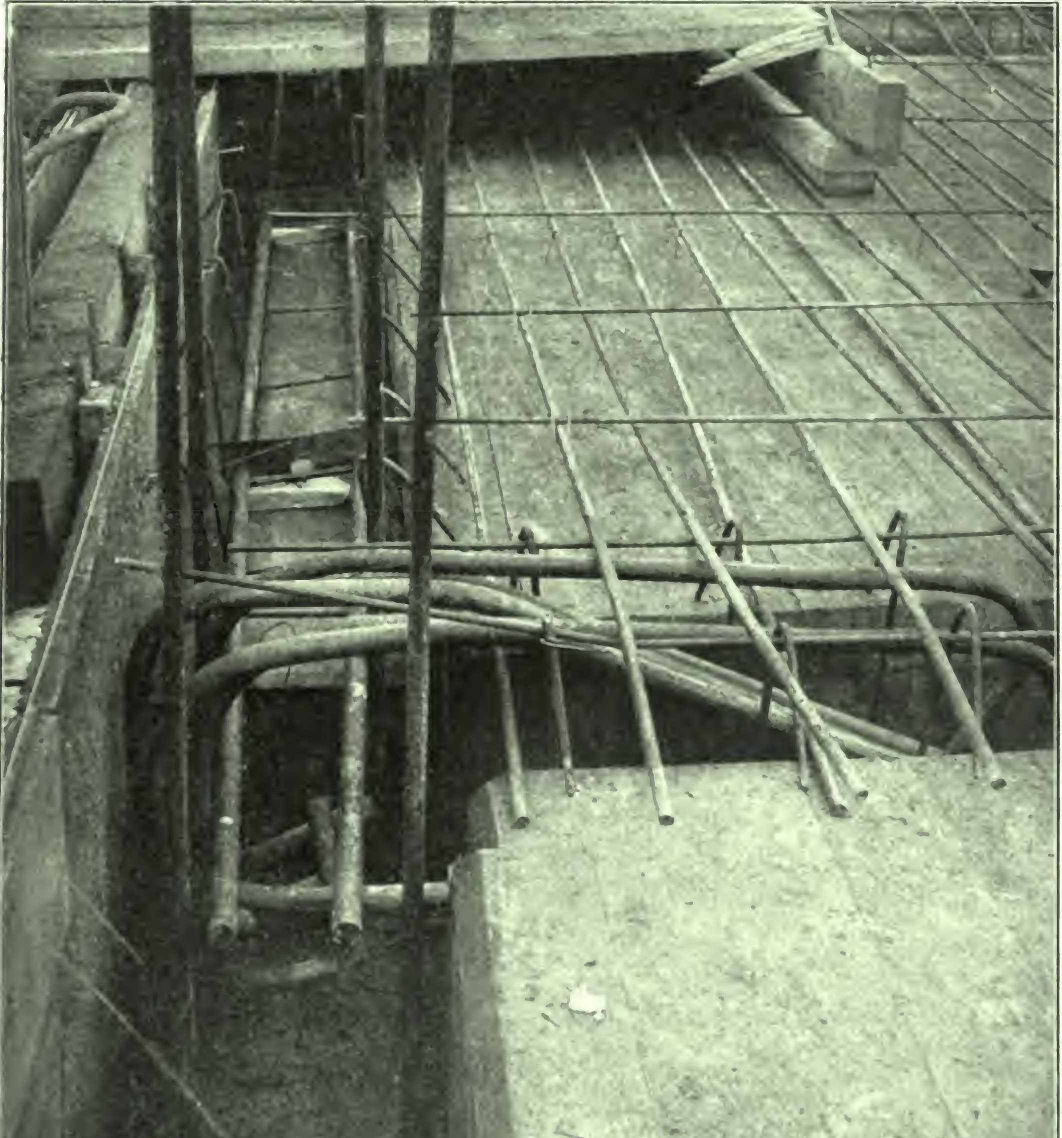

Transactions of the American Society of Civil Engineers

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TRANSACTIONS

OF THE

AMERICAN SOCIETY

OF

CIVIL ENGINEERS

(INSTITUTED 1852)

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AMERICAN SOCIETY OF CIVIL ENGINEERS

INSTITUTED 1852

TRANSACTIONS

Paper No. 1167

EXPANSION OF PIPES.

By Ralph C. Taggart, Assoc. M. Am. Soc. C. E.

With Discussion by Messrs. William D. Ennis, William Kent, and Ralph C. Taggart.

In the arrangement of steam piping (or other piping, the temperature of which is subject to considerable change), proper allowance must be made for expansion. Where the change in temperature, and hence the amount of expansion, is small, the stress may come well within the elastic limit of the metal. In such cases, of course, special arrangements to care for the expansion may not be required.

The calculation to determine the allowable stress in pipe may be readily made. In the case of ordinary iron pipe, we have the following:

The modulus of elasticity of wrought iron, or the stress divided by the strain, equals 29 000 000.

The coefficient of expansion of wrought iron, or the increase in length per degree Fahrenheit per unit length, is 0.00000673.

The stress per degree Fahrenheit, therefore, would be 29 000 000 times 0.00000673, which is equal to 195.2 lb. per sq. in. per degree Fahrenheit difference in temperature. For a change in temperature of 100° Fahr., the stress would become 19 520 lb. per sq. in., which is more than the safe working stress in the iron, especially when it is considered that the stress would be largely increased at the various screw joints, where the thickness of the pipe is reduced by the depth of the thread.

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For ordinary steam apparatus the change in temperature is at least 150° Fahr., so that it becomes impossible for the elasticity of the metal to care for the expansion, even if the piping is very securely tied down. For, when the elastic limit of the metal is reached, a permanent set will result, and if this change in the form of the piping is repeated, a rupture may be expected.

In steam piping, expansion is cared for by two general methods: First, by the use of so-called expansion joints; and second, by the arrangement of the piping, so that the expansion is cared for by the spring of the piping itself.

In apparatus where the straight runs of pipe have not been too long, the second method has been used almost exclusively, although the allowance for expansion has usually been one of judgment or guesswork, and not a matter of calculation.

Where the expansion has been considerable at any one place, it has been common practice for the designing engineer to resort to the use of so-called expansion joints. There are numerous types of these joints, and although many of them have merit, the writer believes that, for many purposes, there are objections to all types. One of the best-known types is made with one metal cylinder sliding or slipping within another. There is, ordinarily, a packed gland or stuffing-box to prevent leakage. An expansion joint of this type should always be anchored, and the pipe which moves within it should also be anchored at a point some distance from it—the distance being determined by the amount of expansion which this particular joint should care for. If the pipe and expansion joint are not thus anchored, the movement of the pipe and the thrust of the steam pressure may carry the inner cylinder of the expansion joint entirely away from the outer cylinder in which it moves. This type requires more or less packing, and although this may not be an important item if only a few expansion joints are used, and if they can be gotten at readily, nevertheless it becomes very important where an engineer has to look after a number of these joints, or where they cannot be reached with the greatest ease.

In a second type of expansion joint, a circular metal disk is fixed at its outer circumference and attached to the expanding pipe near its center. The expansion is taken care of by the spring in the metal disk, and, for this reason, the amount is usually quite small.

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A third type of expansion joint is made up of what may be described as a copper pipe with deep corrugations, reinforced with steel rings. Under certain conditions this joint has been very unsatisfactory. Where it has been subjected to varying temperatures, as, for example, in a heating apparatus where the steam pressure is more or less intermittent, the movement in the copper has resulted in breaking at the corrugations. It is claimed, however, that some good results have been obtained where the steam pressure was not very high, and where the pressure and temperature have been very constant.

Some authorities have suggested the use of fittings arranged so that the expansion will be cared for by the twisting of the pipe within the thread of the fitting. This has been done in some cases in low-pressure work, but a little thought or experience will convince one that it is not a method to be relied on, for as soon as the slightest actual twist occurs within the fitting, the pipe becomes loose, and the joint formed by any white lead or varnish is broken. This destroys the effect of the white lead or varnish, and the difficulty of making an ordinary pipe joint tight without some such cement is well known. In many cases, where it is thought that the expansion is cared for by a twisting in a fitting, a careful examination will show that it is really cared for entirely by the spring of the pipe, and it may be set down as a safe rule that, if there is actually a twist in the pipe-thread, due to expansion, there will almost surely be a leak, even where the pressures are low.

It may be interesting, here, to mention what is known as water packing. A so-called steam-tight joint is sometimes made where one piece of metal slips within another, a few circular rings or grooves being cut in one of the cylinders. The fit, of course, must be very good, and the idea is that the condensed steam in the rings or grooves forms a sort of packing. This arrangement is used with engine indicators and with some reducing-pressure valves of the piston type, where a steam-tight joint is desired and where one cylinder must slip within the other. The success of the joint depends on two things: First, and principally, on very accurate workmanship; and second, on the fact that if a very little steam passes through the joint, any part of it which is condensed will evaporate immediately and pass away unnoticed. This is very soon proven, if the discharge from a reducing-pressure valve of this type is closed, and the line leading to it fills with

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water, when it will be seen that water is leaking from the joint. This is one reason for the old saying that it is easier to make a joint steam-tight than water-tight.

The most common way in which expansion is cared for in steam piping is by the spring or bending of the pipes, where a change in direction occurs, and, on the whole, this method is the most satisfactory. The allowance to be made for expansion, or the length of the spring pieces, however, is usually guessed at, or is determined by experience, rather than by accurate calculation.

Some years ago, the writer made calculations of the lengths of spring pieces for a large underground installation, and, from these calculations, he made a number of diagrams, which he has used to a considerable extent since that time. More recently, however, the original calculations have been somewhat extended, and this paper contains the resulting diagrams and curves, both new and old, together with a short explanation of their derivation and use. It is believed that they will be of value to designing engineers and others.

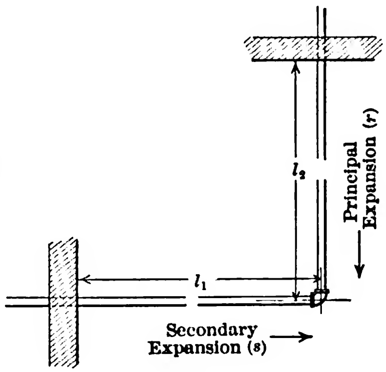

FIG. 1.

Fig. 1 represents two lengths of pipe, l_1 and l_2, connected by a 90° elbow. The lengths, l_1 and l_2, are supposed to represent the distances from the elbow to the points at which the pipe is held in line, or at which the pipe, if horizontal or vertical before expansion, must remain horizontal or vertical after expansion. It will be assumed that the principal expansion acts in a direction at right angles to l_1, and that the secondary or smaller relative expansion, if any, acts at right angles to l_2. Consider, first, a condition in which the secondary expansion is zero. The expansion is then at right angles to l_1, and while the spring in the length of pipe, l_1, must care largely for the expansion, the length, l_2, is also a determining factor. If l_2 becomes zero, or if the pipe at both ends of the length, l_1, is held horizontally, it is easy to determine the length of l_1 required for any given expansion, when the size of the pipe is known.

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Under these conditions the formula may be worked out, and will be found to be as follows:

$$l_1^2 = \frac{87\,000\,000\,D\,r}{f} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots (1)$$

where the modulus of elasticity is taken as that of wrought iron or steel, *viz.*, 29 000 000.

Where l_1 = the length of pipe under strain, in inches;
r = the expansion, in inches, at right angles to the length of pipe, l_1;
D = the outside diameter of pipe, in inches; and,
f = the maximum fiber stress, in pounds per square inch.

This does not allow for the weakening of the pipe at the fitting.

In the case under consideration, the maximum strain occurs at both ends of the length, l_1, of the pipe, and therefore the lessening in strength at the elbow or fitting should be considered, and allowance made therefor. For example, if the pipe is weaker than the fitting at this point, and if the pipe-threads cut into or reduce the effective section of the pipe to two-thirds of its normal section, the strain calculated should be reduced to two-thirds. The same result is accomplished by calculating for the usual strain, with an increase in expansion to one and one-half, for the reason that the maximum fiber stress varies directly as the expansion.

In this connection it is useful to note the following relations which hold true in the equation, $l_1^2 = \dfrac{87\,000\,000\ D\,r}{f}$, and also, in general, in other similar equations which will be developed later.

Other quantities remaining constant (f varies directly as r), or for a fixed length and size of pipe, the maximum fiber stress varies directly as the amount of expansion.

Other quantities remaining constant (f varies directly as D), or for a fixed length and expansion of pipe, the maximum fiber stress varies directly as the diameter of the pipe.

Other quantities remaining constant (r varies inversely as D), or for a fixed length and maximum fiber stress, the expansion varies inversely as the outside pipe diameter.

Other quantities remaining constant (f varies inversely as l^2), or for a fixed pipe diameter and expansion, the maximum fiber stress varies inversely as the square of the length.

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Other quantities remaining constant (r varies directly as l^2), or for a fixed pipe diameter and maximum fiber stress, the expansion varies directly as the square of the length.

If the length, l_2, is to be considered, as well as the length, l_1, the solution of the problem becomes much more complex, but it can be worked out in a manner similar to the solution of the problem of a continuous girder. The solution is given in the following discussion.

Consider first a pipe with two lengths, l_1 and l_2, at right angles, joined together with an elbow at a. The lengths, ac and ad, or l_1 and l_2, are supposed to represent the distances from the elbow to the points at which the pipes pass through walls or are otherwise held at all times in line. Consider now that an expansion occurs in the pipes, with a

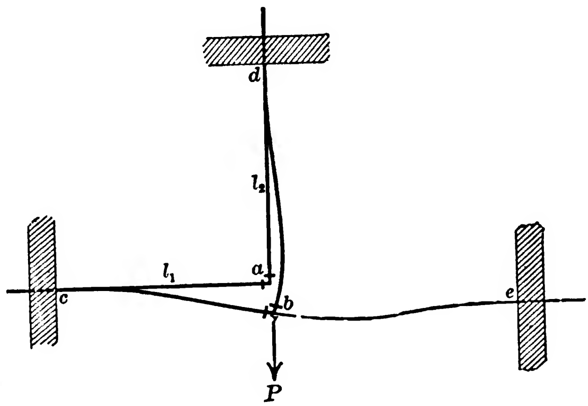

FIG. 2.

slight movement, if necessary, through the two restraining walls, so that the pipes assume the new position, c-b-d. We will assume the principal expansion to be at right angles to l_1, or in the direction, l_2, and the secondary or smaller relative expansion will be at right angles to l_2, or in the direction, l_1. The secondary expansion need not necessarily be less in quantity than the principal expansion, but it is usually less than $\left(\frac{l_2}{l_1}\right)^2$ times the principal expansion. The reason for this will become more apparent as the discussion proceeds, but, of course, it is due to the fact that the expansion largely cared for by l_1 is that at right angles to l_1, and, similarly, the expansion largely cared for by l_2 is that at right angles to l_2, and also because the expansion possible varies as the square of the length of pipe under strain.

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Now consider the length, b-d, swung through 90°, with the point, b, as a center. It will assume the new position, b-e. This will change in no way the conditions of stress, if the elbow is considered as a part of the pipe, and it will give an arrangement to which the formula for continuous girders can easily be applied. The walls at c and e are points of support, and the pipes may be considered as horizontal at these points.

The unknown load, P, will act at b. The difference in elevation between c and b, will be called r, and the difference in elevation between b and e, will be called s. The principal expansion is then equal to r, and the secondary expansion to s. The total horizontal length between c and e will also be considered as $l_1 + l_2$. It is, in fact, practically $l_1 + l_2 + s$, but since s is ordinarily a negligible quantity, as compared with l_1 and l_2, it will be neglected in this connection, although it may be considered in any special case, if desired.

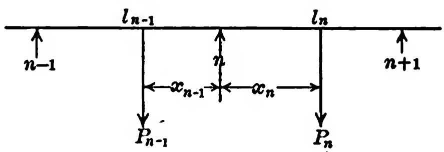

FIG. 3.

The three-moment equation for continuous girders, with not more than one concentrated load on each span, may be written:

$$\frac{M_n}{3}(l_n + l_{n-1}) + M_{n-1}\frac{l_{n-1}}{6} + M_{n+1}\frac{l_n}{6} + \frac{P_n a_n}{6\,l_n}(l_n^2 - a_n^2) + \frac{P_{n-1} a_{n-1}}{6\,l_{n-1}}(l^2_{n-1} - a^2_{n-1})$$
$$= E\,I\left(\frac{Y_{n+1} - Y_n}{l_n} + \frac{Y_{n-1} - Y_n}{l_{n-1}}\right) \ldots\ldots\ldots\ldots\ldots (2)$$

M_{n-1} = moment at support, $n - 1$;
M_n = moment at support, n;
M_{n+1} = moment at support, $n + 1$;
P_{n-1} = concentrated load on span, l_{n-1};
P_n = concentrated load on span, l_n;
l_{n-1} = distance between supports, $n - 1$ and n;
l_n = distance between supports, n and $n + 1$;
X_n = distance from origin to point of application of P_n;
X_{n-1} = distance from origin to point of application of P_{n-1};

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$$a_n = l_n - X_n;$$
$$a_{n-1} = l_{n-1} - X_{n-1};$$
E = modulus of elasticity;

I = moment of inertia of section;

Y_{n+1}, Y_n, and Y_{n-1} = ordinates of points of support, $n+1$, n, and $n-1$.

In this case first assume, as in Fig. 4, that n is at c and $n+1$ at e, noting that the origin is at n.

l_{n-1} and l_{n+1} will then equal zero.

Let h equal the difference in elevation between c and e or $(r - s)$.

In all cases the moment at c will be called M_1, and the moment at e, M_2. Then, from Equation 2:

$$\frac{M_1 l_n}{3} + \frac{M_2 l_n}{6} + \frac{P_n a_n}{6 l_n}(l_n^2 - a_n^2) = -E I \frac{h}{l_n} \quad \ldots\ldots\ldots\ldots(3)$$

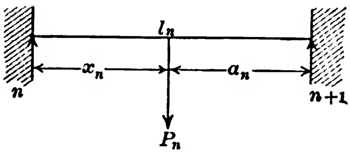

FIG. 4.

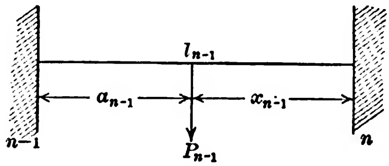

FIG. 5.

If we now assume, as in Fig. 5, that $n-1$ is at c and n at e, noting again that the origin is at n, we will have, from Equation 2:

$$\frac{M_2 l_{n-1}}{3} + \frac{M_1 l_{n-1}}{6} + \frac{P_{n-1} a_{n-1}}{6 l_{n-1}}(l^2_{n-1} - a^2_{n-1})$$
$$= E I \frac{h}{l_{n-1}} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(4)$$

Substituting, in Equation 4, the values used in Equation 3 for l_{n-1}; P_{n-1}; a_{n-1}; *viz.*, l_n; P_n; $l_n - a_n$, we have:

$$\frac{M_2 l_n}{3} + \frac{M_1 l_n}{6} + \frac{P_n a_n}{6 l_n}(2 l_n^2 - 3 a_n l_n + a_n^2) = E I \frac{h}{l_n} \quad \ldots\ldots\ldots(5)$$

If we make L equal to l_n; P equal to P_n; and A equal to a_n, Equations 3 and 5 will then reduce to

$$2 M_1 L + M_2 L + \frac{PA}{L}(L^2 - A^2) = -6 E I \frac{h}{L} \quad \ldots\ldots\ldots(6)$$

$$M_1 L + 2 M_2 L + \frac{PA}{L}(2 L^2 - 3 A L + A^2) = 6 E I \frac{h}{L} \ldots(7)$$

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Whence, by multiplying Equation 6 by 2 and subtracting Equation 7:

$$3\,M_1\,L + \frac{2\,P\,A}{L}\,(L^2 - A^2) - \frac{P\,A}{L}\,(2\,L^2 - 3\,A\,L + A^2) = -\,18\,E\,I\,\frac{h}{L}$$

or

$$M_1 = -\,6\,E\,I\,\frac{h}{L^2} - \frac{P\,A^2(L - A)}{L^2} \quad \ldots\ldots\ldots\ldots\ldots (8)$$

In a similar manner, by multiplying Equation 7 by 2 and subtracting Equation 6:

$$3\,M_2\,L + \frac{2\,P\,A}{L}\,(2\,L^2 - 3\,A\,L + A^2) - \frac{P\,A}{L}\,(L^2 - A^2) = 18\,E\,I\,\frac{h}{L}$$

or

$$M_2 = 6\,E\,I\,\frac{h}{L^2} - \frac{P\,A}{L^2}\,(L - A)^2 \quad \ldots\ldots\ldots\ldots\ldots (9)$$

Then Equations 8 and 9 give the moments at the two points of support.

The shear just at the right of the support at c, may be expressed as follows:

$$F = \frac{M_2 - M_1 + P\,A}{L} \quad \ldots\ldots\ldots\ldots\ldots\ldots (10)$$

Substituting the values of M_1 and M_2, as shown in Equations 8 and 9,

$$F = \frac{1}{L}\left(6\,E\,I\,\frac{h}{L^2} - \frac{P\,A}{L^2}\,(L - A)^2 + 6\,E\,I\,\frac{h}{L^2} + \frac{P\,A^2(L - A)}{L^2} + P\,A\right)$$

or

$$F = 12\,E\,I\,\frac{h}{L^3} + \frac{P\,A^2}{L^3}\,(3\,L - 2\,A) \ldots\ldots\ldots\ldots (11)$$

The three-moment Equation 2 is derived fundamentally from two equations (Figs. 3 and 4), namely,

When $X < X_n;\ M = M_n + F_n\,X \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (12)$

When $X > X_n;\ M = M_n + F_n\,X - P_n\,(X - X_n) \ldots\ldots\ldots (13)$

When M equals the moment at any point, F_n equals the shear at the right of the support, n.

Since $$\frac{d^2\,y}{d\,X^2} = \frac{M}{E\,I};\ M = \frac{d^2\,y}{d\,X^2}\,E\,I.$$

Substituting this value in Equations 12 and 13, integrating and determining constants, we will have:

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Where α_1 equals slope distance, X, to right of origin, α_n equals value of α_1, when X equals 0.

For $$X < X_n;\ E I \frac{d y}{d X} = E I \tan. \alpha_n + M_n X + F_n \frac{X^2}{2} \quad \text{......(14)}$$

$$X > X_n;\ E I \frac{d y}{d X}$$

$$= E I \tan. \alpha_n + M_n X + F_n \frac{X^2}{2} - \frac{P_n (X - X_n)^2}{2} \quad \text{.......(15)}$$

Integrating again, and determining constants, we have:

For $$X < X_n;\ E I y = E I X \tan. \alpha_n + M_n \frac{X^2}{2} + F_n \frac{X^3}{6} \quad \text{.....(16)}$$

$$X > X_n;\ E I y$$

$$= E I X \tan. \alpha_n + M_n \frac{X^2}{2} + F_n \frac{X^3}{6} - \frac{P_n (X - X_n)^3}{6} \quad \text{.......(17)}$$

If we now give X the value X_n, and Y the value $-r$, in either Equations 16 or 17 (*viz.*, substitute the value of X and Y at the point of application of P), we will have:

With $\tan. \alpha_n = 0$

$$-E I r = M_n \frac{X_n^2}{2} + F_n \frac{X_n^3}{6} \quad \text{...............(18)}$$

In this equation make M_n equal to the value of M_1 in Equation 8; make F_n equal to the value of F in Equation 11, and substitute for X_n the value $L - A$. We will then have:

$$-E I r = \left(-6 E I \frac{h}{L^2} - \frac{P A^2 (L - A)}{L^2}\right) \frac{(L - A)^2}{2}$$

$$+ \left[12 E I \frac{h}{L^3} + \frac{P A^2}{L^3} (3 L - 2 A)\right] \frac{(L - A)^3}{6} \quad \text{..... (19)}$$

or simplifying,

$$E I r = \frac{P A^3}{3 L^3} (L - A)^3 + \frac{E I h}{L^3} (L^3 - 3 A^2 L + 2 A^3) \quad \text{...(20)}$$

This gives the following value for P:

$$P = \frac{3 L^3 E I r - 3 E I h (L^3 - 3 A^2 L + 2 A^3)}{A^3 (L - A)^3} \quad \text{.......(21)}$$

The bending moment will be a maximum at the wall, c, where the bending moment is M_1, therefore,

$$\frac{f I}{\frac{1}{2} D} = M_1 \quad \text{........................(22)}$$

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Substituting the value of M_1 from Equation 8:

$$\frac{f\,I}{\frac{1}{2}\,D} = -6\,E\,I\,\frac{h}{L^2} - \frac{P\,A^2\,(L-A)}{L^2} \text{ .... } (23)$$

From which:

$$P = \frac{-\frac{L^2\,f\,I}{\frac{1}{2}\,D} - 6\,E\,I\,h}{A^2\,(L-A)} \text{ .... } (24)$$

Equating the values of P, as found in Equations 21 and 24, we have:

$$\frac{3\,L^3\,E\,I\,r - 3\,E\,I\,h\,(L^3 - 3\,A^2\,L + 2\,A^3)}{A^3\,(L-A)^3}$$

$$= \frac{-\frac{L^2\,f\,I}{\frac{1}{2}\,D} - 6\,E\,I\,h}{A^2\,(L-A)} \text{ .... } (25)$$

Simplifying

$$-3\,E\,r = \frac{2\,f\,A}{D\,L}(L-A)^2 - \frac{3\,E\,h}{L^2}(L-A)^2 \text{ .... } (26)$$

or

$$r = -\frac{2\,f\,A\,(L-A)^2}{3\,D\,E\,L} + \frac{h}{L^2}(L-A)^2 \text{ .... } (27)$$

If we make $k\,L = X_n$ and $L - k\,L = A$, then $A = (1-k)\,L$ and, substituting in Equation 27, we have:

$$r = -\frac{2\,f\,(1-k)\,L^2\,k^2}{3\,D\,E} + h\,k^2 \text{ .... } (28)$$

If we substitute the value $(r - s)$ for h, we will have:

$$r = \frac{-2\,f\,(1-k)\,L^2\,k^2}{3\,D\,E} + (r-s)\,k^2 \text{ .... } (29)$$

or

$$r\,(1-k^2) + s\,k^2 = -\frac{2\,f\,(1-k)\,L^2\,k^2}{3\,D\,E} \text{ .... } (30)$$

Or since $L\,k = l_1$

$$r\,(1+k) + s\,\frac{k^2}{1-k} = \frac{-2\,f\,l_1^2}{3\,D\,E} \text{ .... } (31)$$

Where r = principal expansion, in inches;
s = secondary expansion, in inches;
$k = \frac{l_1}{l_1 + l_2}\left(\text{or } \frac{l_1}{L}\right)$;
$L = l_1 + l_2$ (all in inches);
f = maximum fiber stress (pounds per square inch);
D = outside diameter of pipe, in inches; and,
E = modulus of elasticity.

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If s equals 0, that is, if the secondary expansion is zero, then:

$$r\,(1 + k) = -\frac{2\,l_1^2 f}{3\,D\,E} \qquad (32)$$

If $k = 1$; $l_2 = 0$; and $r = -\frac{l_1^2 f}{3\,D\,E}$, or

$$l_1^2 = \frac{-3\,D\,E\,r}{f} = \frac{-87\,000\,000\,D\,r}{f} \qquad (33)$$

which is the same as Equation 1.

This is the condition of a beam, both ends of which are held horizontal while one end is forced to a lower level than the other.

If, on the other hand, s equals zero and k approaches zero (which shows that l_2 becomes very long as compared with l_1), then r approaches a value $-\frac{2\,l_1^2 f}{3\,D\,E}$, and would become equal to it in the limit. This is, of course, the condition of a beam fixed at one end and free at the other. In this case the length, l_2, would act as if it (the pipe) were cut off at the elbow. It should be noted that the value of r, when k equals zero, or l_2 equals infinity, is twice that when k equals 1, or l_2 equals zero.

From Equation 31 certain curves have been worked out, which can be used when it is not desired to solve the equation for each independent case. The method of using these curves will be shown for a number of cases.

The curves, Figs. 6 to 22, are calculated (for wrought-iron or steel pipe), for a fixed expansion in a direction at right angles to l_1, which will be called the principal expansion, and, for a zero expansion, in the direction of l_1 or at right angles to l_2, which will be called the secondary expansion. If the secondary expansion must be considered, it may be calculated from the equations which have been derived, or one may make use of the curves, Figs. 23 and 24, which are to be used in connection with the curves, Figs. 6 to 22, as will be explained later. It will be noted that the lengths of pipe, l_1 and l_2, are given in feet in all the diagrams, although, in the equations, the units are in inches.

Consider first the expansion in a 4-in. pipe, where the principal expansion is 6 in., and where the secondary expansion (or that at right angles to the principal expansion), is negligible. This length can be determined directly from Fig. 13. If l_2 is zero, the length of l_1 will be 37 ft. If l_2 equals 10 ft., it will be more than $\frac{l_1}{4}$, because l_1 must

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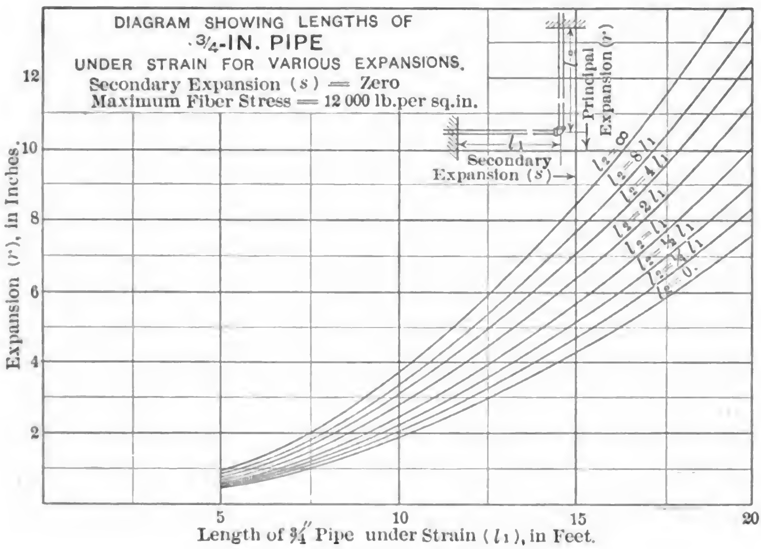

Fig. 6.

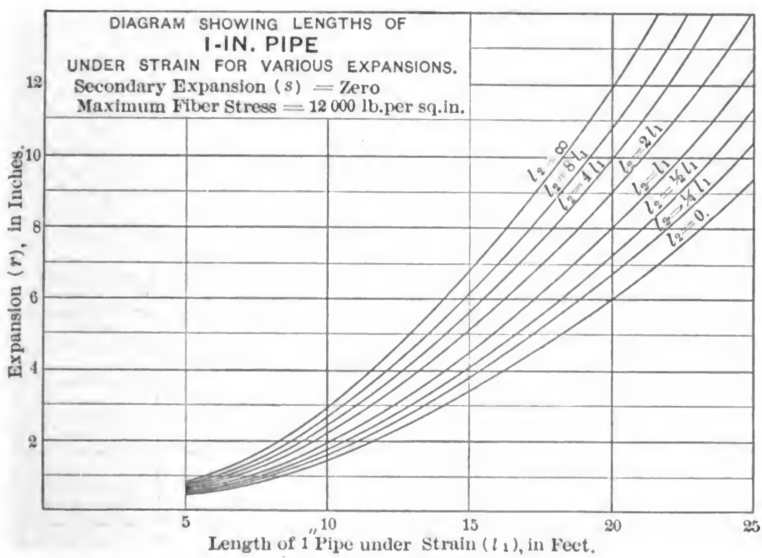

Fig. 7.

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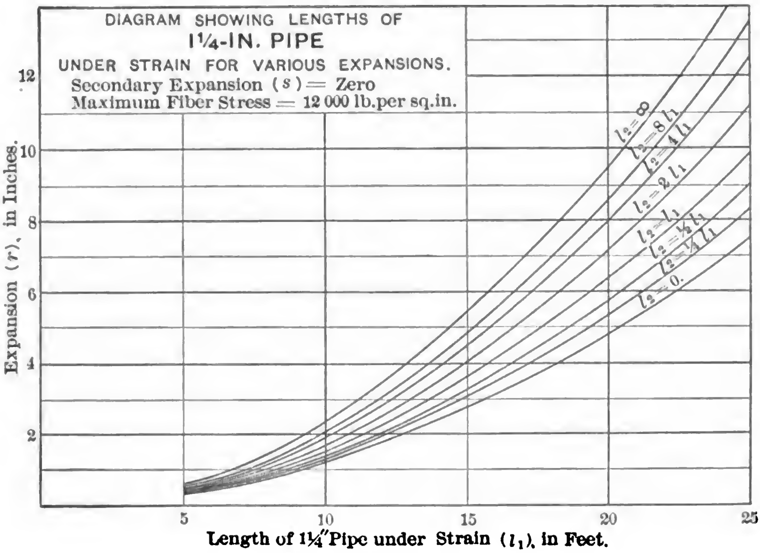

FIG. 8.

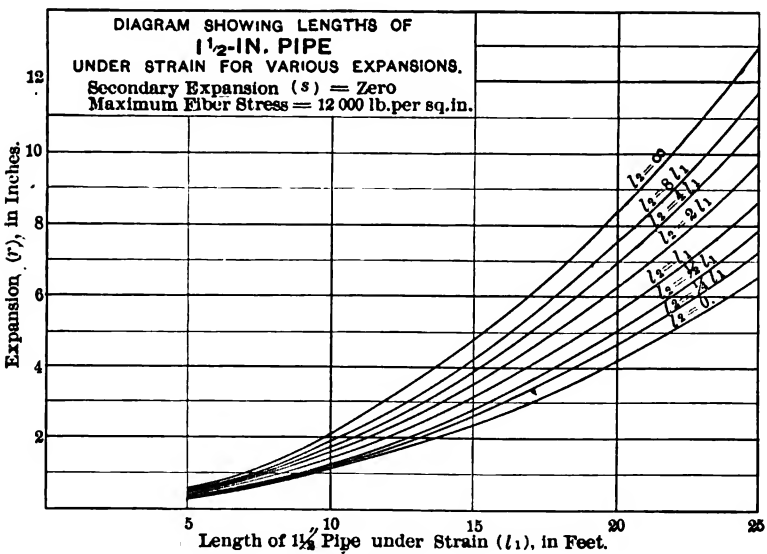

FIG. 9.

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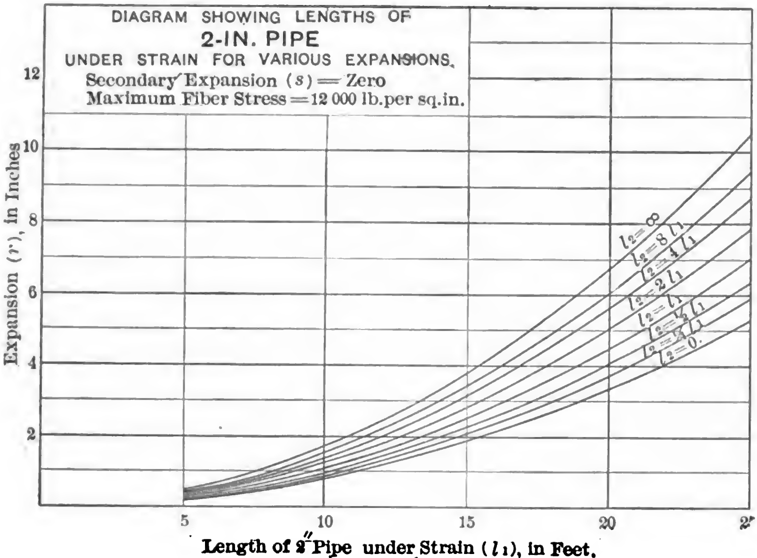

FIG. 10.

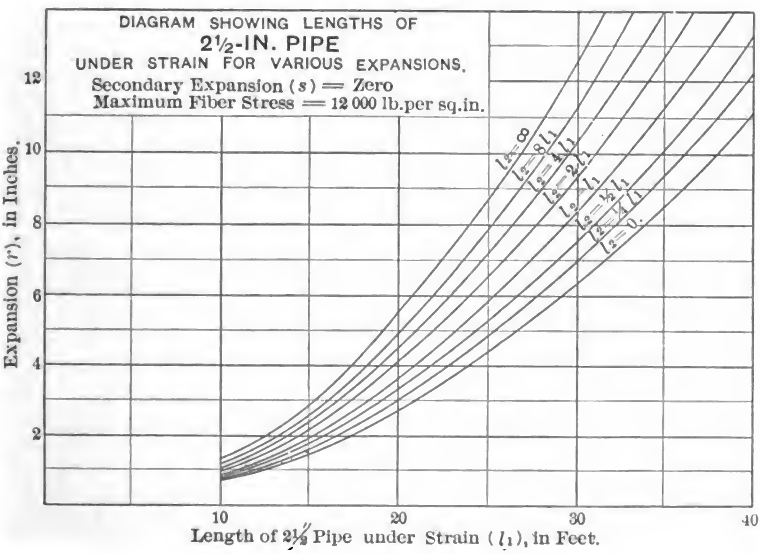

FIG. 11.

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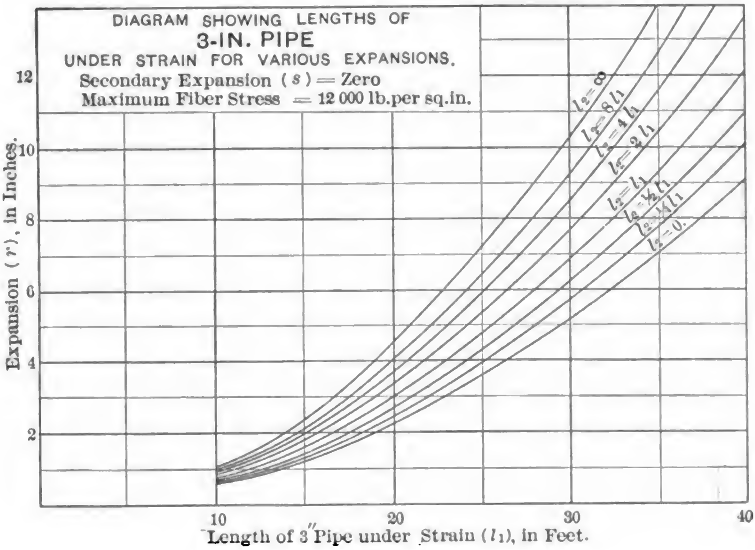
DIAGRAM SHOWING LENGTHS OF
3-IN. PIPE
UNDER STRAIN FOR VARIOUS EXPANSIONS.
Secondary Expansion (s) = Zero
Maximum Fiber Stress = 12 000 lb.per sq.in.
Expansion (r), in Inches.
Length of 3" Pipe under Strain (l1), in Feet.

Fig. 12.

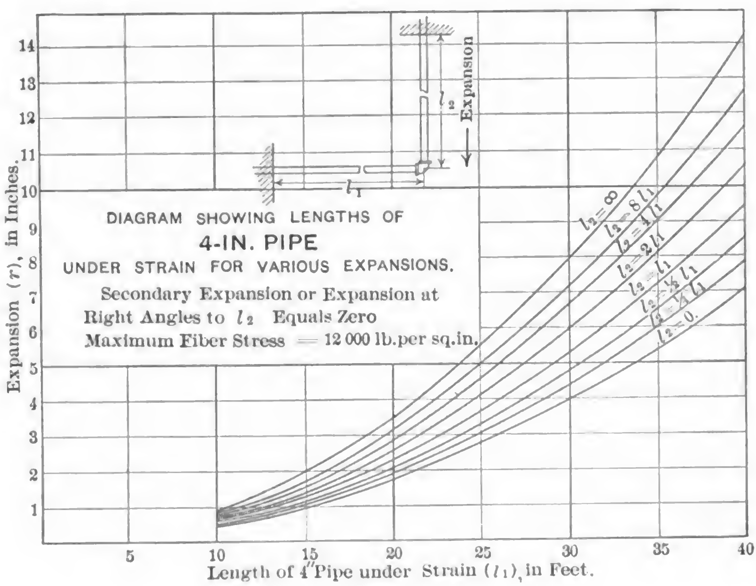
DIAGRAM SHOWING LENGTHS OF
4-IN. PIPE
UNDER STRAIN FOR VARIOUS EXPANSIONS.
Secondary Expansion or Expansion at
Right Angles to l2 Equals Zero
Maximum Fiber Stress = 12 000 lb.per sq.in.
Expansion
Expansion (r), in Inches.
Length of 4" Pipe under Strain (l1), in Feet.

Fig. 13.

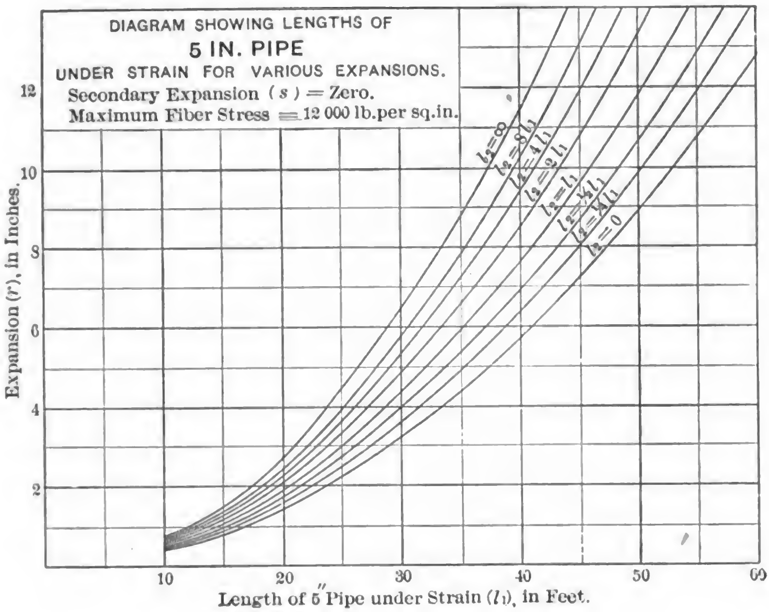

FIG. 14.

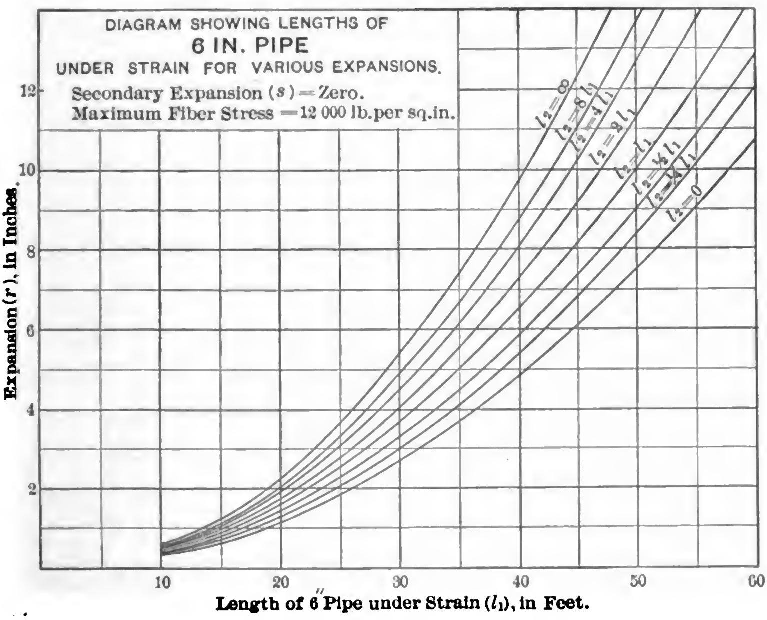

FIG. 15.

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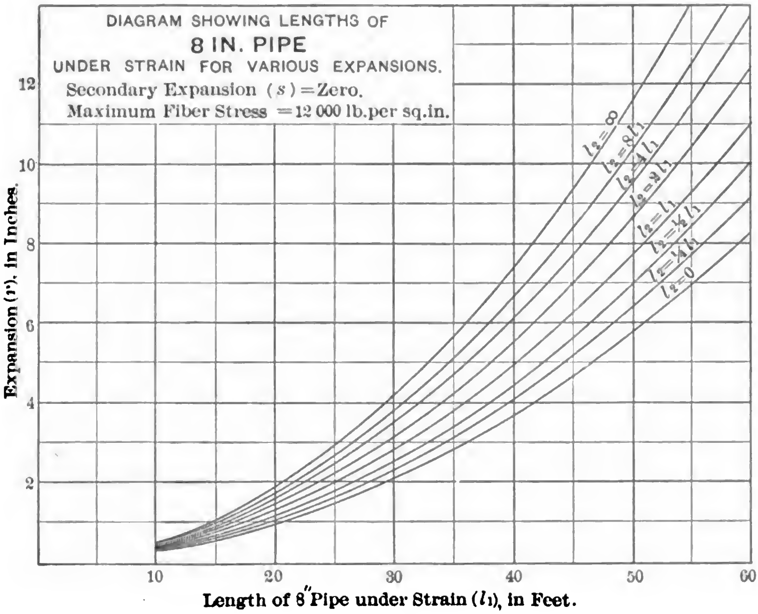

FIG. 16.

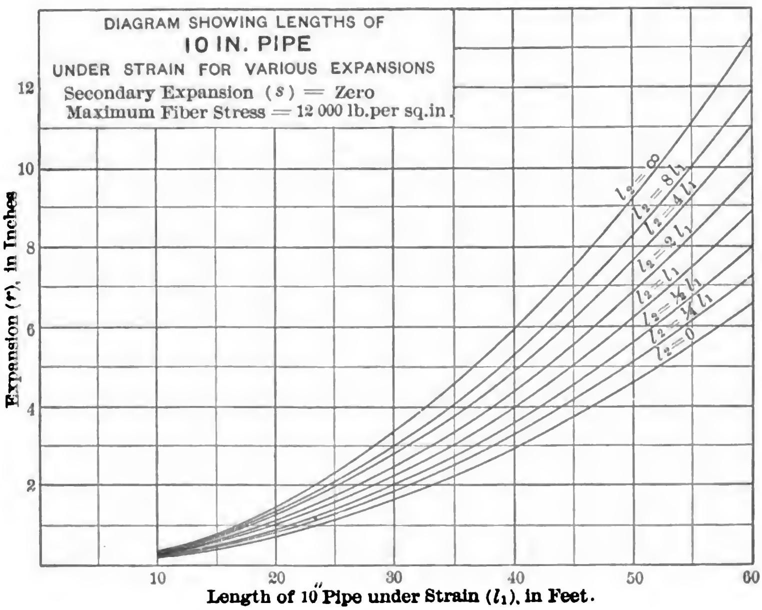

FIG. 17.

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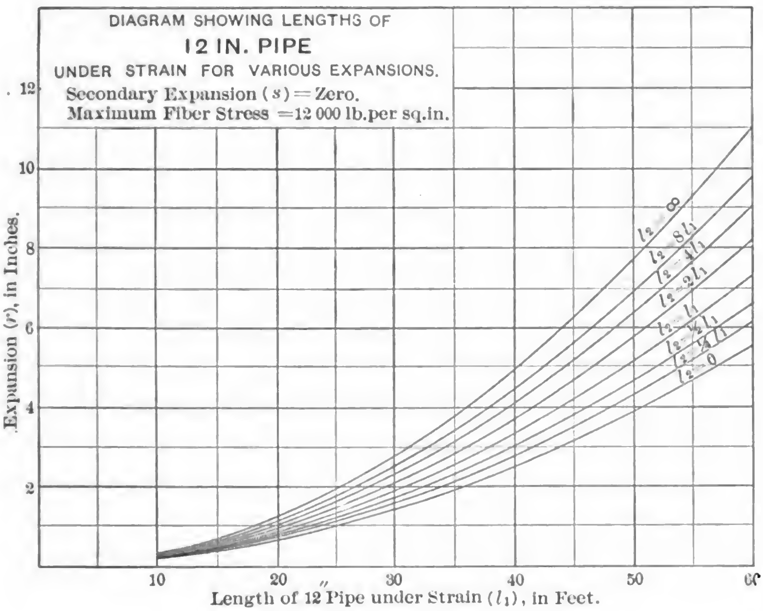

Fig. 18.

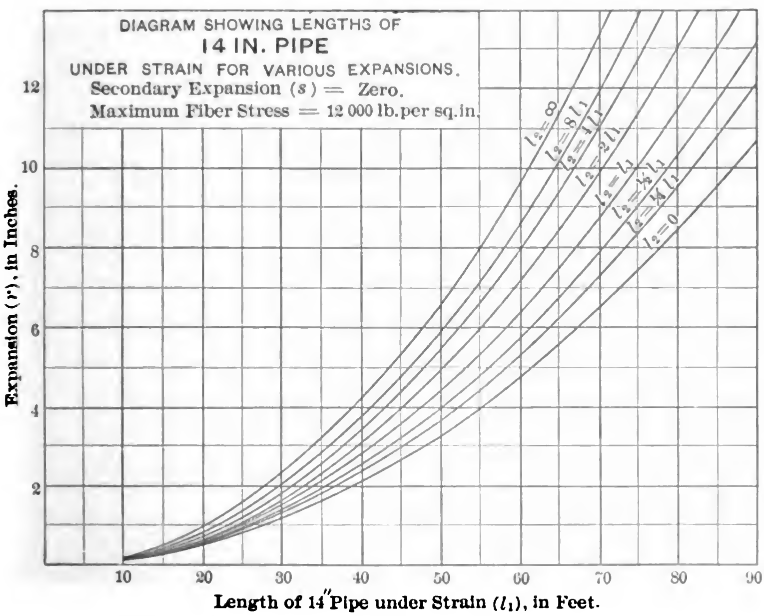

Fig. 19.

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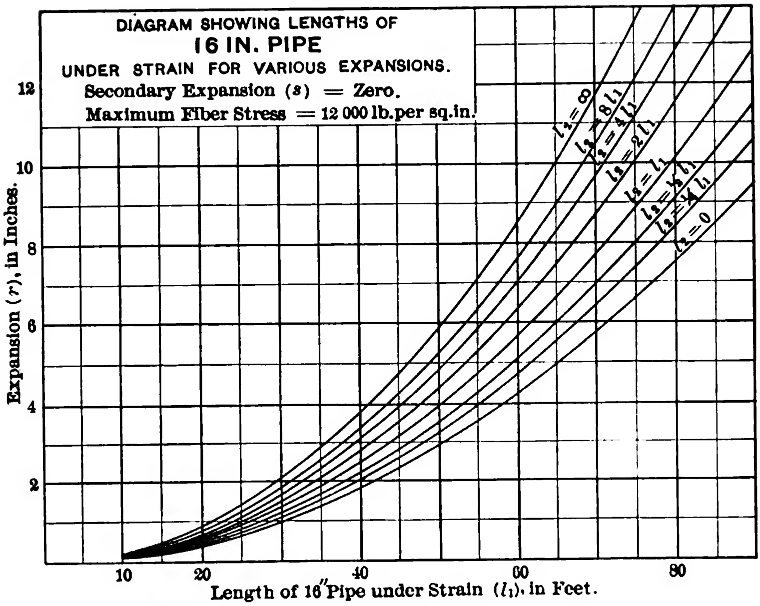

FIG. 20.

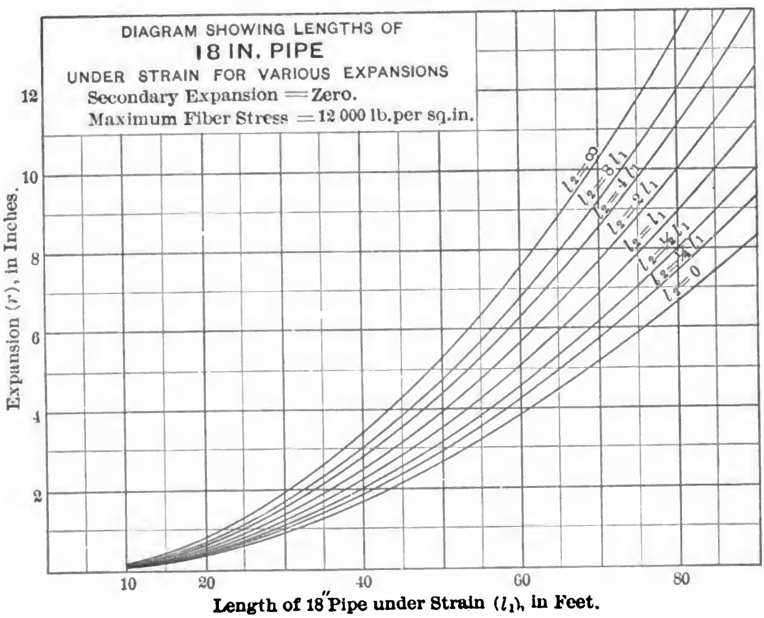

FIG. 21.

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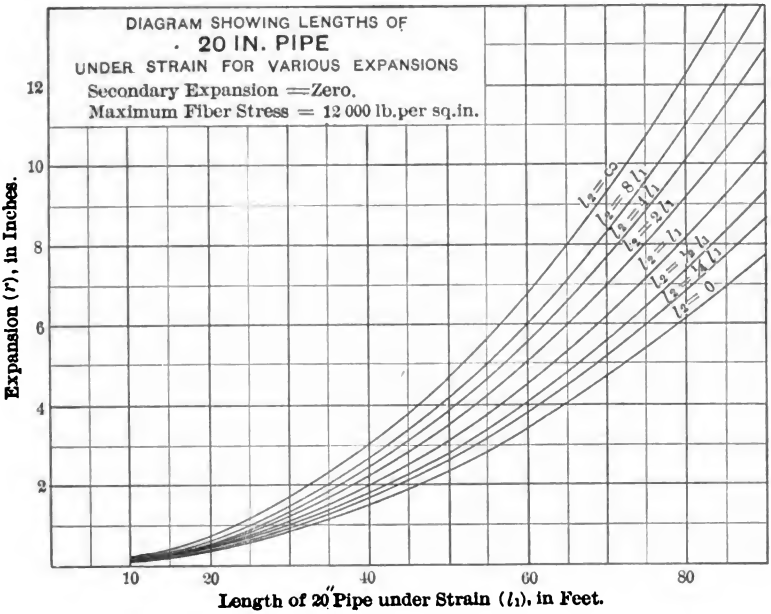

Fig. 22.

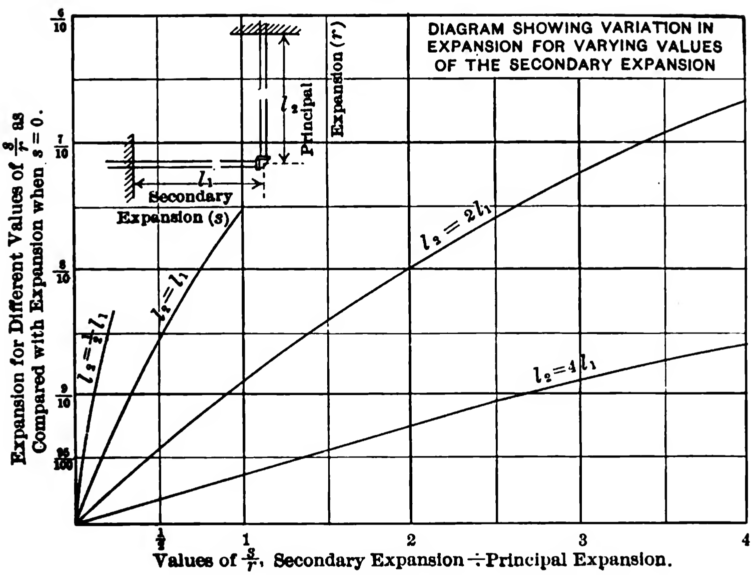

Fig. 23.

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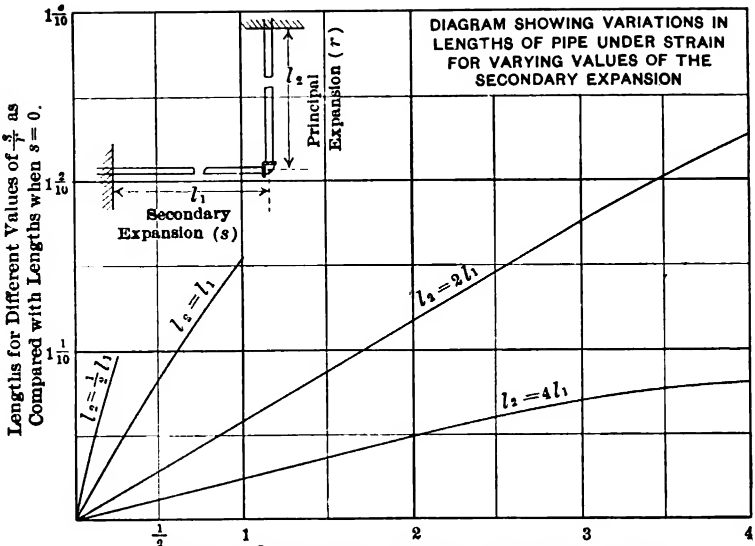

Fig. 24.

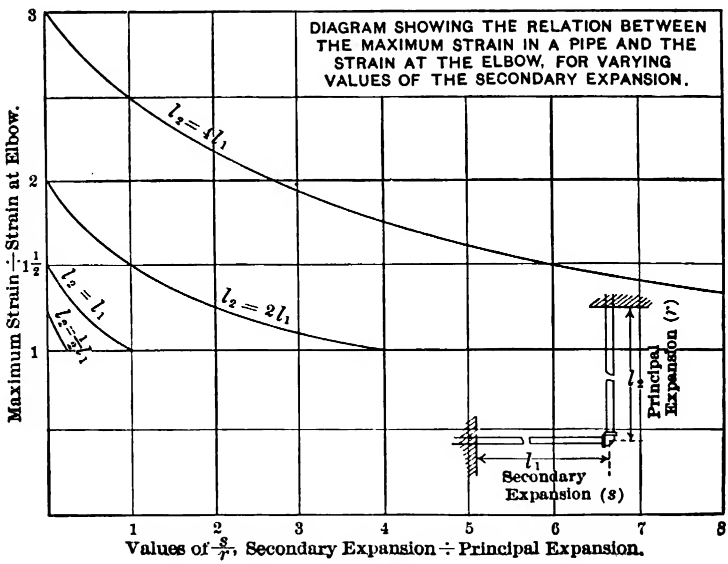

Fig. 25.

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equal something less than 37 ft., if l_2 has any value at all. With l_2 equal to 10 ft., l_2 is more than $\frac{l_1}{4}$ and undoubtedly less than $\frac{l_1}{2}$. If l_2 equals $\frac{l_1}{2}$, it will be seen from Fig. 13, that l_1 will be a little less than 34 ft., and if l_2 equals $\frac{l_1}{4}$, l_1 equals 35 ft. It will be seen, therefore, that l_2 is about two-sevenths of l_1 or nearly $\frac{l_1}{4}$, and that l_1 should be taken as 34½ or 35 ft.

If the length, l_2, becomes equal to 8 l_1, it will be seen from Fig. 13 that l_1 should equal 27½ ft. When l_2 becomes very long, the fact as to whether it can move freely must, of course, be taken into consideration. If it must slip over fixed supports, the friction of slippage will, of course, have the same effect as the shortening of the length, l_2. On the other hand, it often happens that pipes passing through walls, or in similar conditions, will be held so as to permit of some slight side movement, although it may not be allowed for in the calculations. This, of course, would have the same effect as an increase in the length of pipe under strain.

Let us now consider the expansion in a 4-in. pipe where the principal expansion is 6 in., the secondary expansion is 3 in., and the lengths of l_1 and l_2 are to be equal. From Fig. 24, it will be seen, when the principal expansion divided by the lesser or secondary expansion $\left(\text{or } \frac{s}{r}\right) = \frac{1}{2}$, and when $l_1 = l_2$, that (compared with the case where the secondary expansion equals zero) the lengths should be increased in the ratio of 1 to 1.085, or, from Fig. 23, it will be seen that the expansion should be decreased in the ratio of 1 to 0.85.

From Fig. 13 it will be seen that l_1 should be nearly 32 ft. for a 4-in. pipe with a 6-in. expansion (the secondary expansion being zero and $l_1 = l_2$). If, in addition to the principal expansion of 6 in., there is a secondary expansion of 3 in., the lengths should be increased, as before stated, in the ratio of 1 to 1.085, or 32 ft. should be changed to nearly 35 ft. If, however, it is desired not to increase these lengths, the lessened amount of expansion or the increased maximum fiber stress can be determined. From Fig. 23 it has been determined that the expansion should be decreased in the ratio of 1 to 0.85, or

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6 and 3 in. should become 5.1 and 2.55 in., respectively. If, however, it is desired to maintain the 6- and 3-in. expansion, it will be seen from the equations that the fiber stress varies directly as the expansion, and hence the maximum fiber stress will become

$$12\,000 \times \frac{1}{0.85} = 14\,118 \text{ per sq. in.}$$

There is one factor which has already been mentioned, to which, however, further attention should be given, that is the question as to how much allowance should be made for the weakening of a pipe at a fitting due either to the strength of the fitting or the threads on the pipe. In order to determine what this allowance should be, we must make $\frac{f\,I}{\frac{1}{2}D}$, in Equation 22, equal to the moment where $X = k\,L$, instead of to M_1, where $X = 0$. Following through similar succeeding equations, we will secure the equation:

$$-r = \frac{f}{3\,E\,D}\,L^2\,k\,(k-1) - h\,k \ldots\ldots\ldots\ldots (34)$$

or

$$r + s\,\frac{k}{1-k} = \frac{f\,l_1^2}{3\,E\,D\,k} \ldots\ldots\ldots\ldots\ldots\ldots (35)$$

The comparative values of the pipe strain at the joint and the fixed point of the pipe, have been determined from Equation 35, and are shown graphically in Fig. 25.

In using this diagram in the following discussion, the loss in strength at the elbow is calculated as one-third. Therefore, an allowance should be made for the weakening at the fitting when the maximum strain is less than $\frac{1}{1-\frac{1}{3}}$, or $1\frac{1}{2}$ times the strain at the elbow. An examination of the curves on Fig. 25 will show that this is practically always the case when $l_2 = l_1$, or anything less than l_1. It is also the case when $l_2 = 2\,l_1$, and the secondary is equal to or larger than the principal expansion. It will be remembered that the principal expansion is usually larger than $\left(\frac{l_1}{l_2}\right)^2$ times the secondary expansion, where l_1 is the length of pipe under strain at right angles to the direction of the principal expansion, and l_2 is the length of pipe under strain at right angles to the secondary expansion, but the principal expansion is not necessarily larger in quantity than the secondary expansion.

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The allowance will depend on the ratio of the maximum strain to the strain at the elbow, which values are shown by the curves in Fig. 25. When $l_2 = 0$, the strain at the elbow equals the strain at the point where the pipe is held in line, and the strain at all intermediate points is also the same. The allowance for the weakening at the elbow, or for any joint between the elbow and the point where the pipe is held in line, therefore, should be in the ratio of 1 to $1\frac{1}{2}$ or the strain allowable should be two-thirds of what otherwise might be calculated when $l_2 = 0$.

If l_2 has some other value, for example, if $l_2 = 2\ l_1$, and if $\frac{s}{r} = 2$, it will be seen, from Fig. 25, that the maximum strain is $1\frac{1}{4}$ times the strain at the elbow. If we calculate the elbow to be only two-thirds as strong as the pipe, the strain in the latter should be reduced to $\frac{2}{3} \times 1\frac{1}{4} \left(= \frac{5}{6}\right)$ of what the pipe might stand, so as not to overstrain the material at the elbow. If there is a fitting half way between the elbow and the point at which the pipe is held in line, the maximum strain would be greater than the strain at this point, in the ratio of 1 to a quantity half way between 1 and $1\frac{1}{4}$, or in the ratio of 1 to $1\frac{1}{8}$.

If the fitting were farther from the elbow, its distance being two-thirds of the total distance between the point at which the pipe is held in line and the elbow, the ratio would be 1 to $\left(1 + \frac{1}{3}\right)\frac{1}{4}$, which is equal to the ratio of 1 to $1\frac{1}{12}$.

When $l_2 = 0$, therefore, the allowance for the weakening at the elbow should be made as follows:

$$\frac{\text{The new } l_1^2 \text{ and } l_2^2}{\text{The old } l_1^2 \text{ and } l_2^2} = \frac{1}{\frac{2}{3}}, \text{ or,}$$

$$\frac{\text{The new } l_1 \text{ and } l_2}{\text{The old } l_1 \text{ and } l_2} = \sqrt{\frac{3}{2}} = \sqrt{1.5} = 1.225,$$

or the new lengths should equal 1.225 times the old lengths.

An examination of the equation and curves will also show that this same rule may be followed closely when the secondary expansion, although not zero, is still small as compared with $\left(\frac{l_2}{l_1}\right)^2$ times the principal expansion, and, in no case, will the allowance be greater than this amount.

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The diagrams may be used for a variety of combinations, and their adaptability is well illustrated by Fig. 26.

An arrangement of piping is assumed, in which a tee connects an 8- and a 6-in. pipe on the run and a 4-in. pipe branches from the tee at right angles to both. The length of the 8-in. pipe from the tee to the point at which it is held in line will be assumed to be 30 ft., and that of the 6-in. pipe will be assumed to be 25 ft. If the movement of the tee in the direction of the 6- and 8-in. pipes is 2 in., what should the length of the 4-in. pipe be from the tee to the point at which it is held in line?

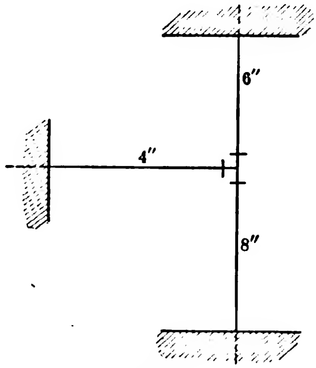

FIG. 26.

It will first be assumed that the expansion of the tee in the direction of the 4-in. pipe is a negligible quantity. From the formula it is seen that the bending for a fixed maximum fiber stress varies inversely as the diameter of the pipe and the square of the length of pipe. The 30 ft. of 8-in. pipe, therefore, could, for a fixed maximum fiber stress, be bent only one-half as much as 30 ft. of 4-in. pipe. Therefore, the 30 ft. of 8-in. pipe is equivalent in its power to bend to 21.2 ft. of 4-in. pipe, since the lengths vary as the square root of the bending power, and $\frac{21.2}{30} = \sqrt{\frac{1}{2}}$. In the same way, the 25 ft. of 6-in. pipe is equivalent to $\frac{25}{\sqrt{1.5}} = \frac{25}{1.224} = 20$ ft. (about).

We have now the double stiffness of an equivalent of 21.2 ft. of 4-in. pipe and 20 ft. of 4-in. pipe. The stiffness is proportional to $\frac{1}{l^2}$, or to the reciprocal of the length squared. Therefore, the combined stiffness of the two 6- and 8-in. pipes would be equivalent to $\frac{1}{(21.2)^2} + \frac{1}{20^2} = \frac{849.44}{179\,776}$, which would be equivalent to a 4-in. pipe, with a length of $\sqrt{\frac{179\,776}{849.44}} = \sqrt{211.64} = 14.54$ ft.

If we use 14.54 as l_2, therefore, in Fig. 13, we can secure the necessary length of 4-in. pipe.

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We see from this diagram that, if $l_2 = 0$, 2 in. of expansion for a 4-in. pipe would require a length of $21\frac{1}{4}$ ft. of 4-in. pipe. If $l_2 = l_1$, l_1 would be $18\frac{1}{8}$ ft., but l_2 is equivalent to 14.5 ft. The next smaller value for l_2 is l_2 equals $\frac{1}{2}\, l_1$. In this case, l_1 would equal $19\frac{3}{8}$ ft. In the first case, if $l_2 = l_1$, l_2 would be $18\frac{1}{8}$. In the second case, if $l_2 = \frac{1}{2}\, l_1$, l_2 would be $9\frac{11}{16}$ ft., but l_2 is actually $14\frac{1}{2}$ ft., or about halfway between the two; therefore, l_1 lies between $19\frac{3}{8}$ and $18\frac{1}{8}$, and would not be far from 19 ft.

If now there should be a secondary expansion of $\frac{1}{2}$ in., it would be necessary to increase the value of l_1. If l_2 could not be increased proportionately, the value of $\frac{l_2}{l_1}$ would be decreased, and instead of being $\frac{14\frac{1}{2}}{19}$, or approximately $\frac{3}{4}$, it would become something less. From Fig. 24 it will be seen that when the secondary expansion divided by the principal expansion, or $\frac{s}{r} = \frac{1}{4}$, and when $l_2 = \frac{1}{2}\, l_1$, that the new length should be about 1.1 of the old length. This, however, would change l_1 so little that the value of $\frac{l_2}{l_1}$, while less than $\frac{3}{4}$, would not be nearly so small as $\frac{1}{2}$. We can, therefore, approximate the value of $\frac{l_2}{l_1}$ as $\frac{9}{10} \times \frac{3}{4} = \frac{27}{40} = 0.675$, or about $\frac{2}{3}$, and we can approximate the required length of l_1 from Fig. 24, as about 1.09 of the old length of 19 ft. or the new length of l_1 would become about $19 \times 1.09 = 20.71$, or about $20\frac{3}{4}$ ft.

Let us now consider a case in which the strain is not caused by an expansion or movement at the tee, and in which the end of the 4-in. pipe farthest from the tee is not held in line. The problem will be to find what bending is possible at the end of the 4-in. pipe farthest from the tee, if this end is not held horizontal or in its original direction. The length of the 8- and 6-in. pipes will be the same as before, and the length of the 4-in. pipe will be the same as in the last case, *viz.*, $20\frac{3}{4}$ ft.

In this case the 8- and 6-in. pipes together have an equivalent length of 14.54 ft. of 4-in. pipe.

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It is apparent that, in this case, the bending will be the same as for a 4-in. pipe of length $(20.7 + 14.54) = 35.24$ ft. with its end free. A free end is a similar condition to $l_2 = \infty$, as the fact of the ends not being free is due to a strain from l_2, and this becomes zero when $l_2 = \infty$. We will find, therefore, the expansion on Fig. 13, and note that it is nearly 11 in.

It is sometimes the practice to use bends. An analysis of the conditions of strain in a 90° bend will show that such a bend has approximately the same strength as a pipe making a 90° turn with an elbow, the two sides of which are each equal to the radius of the bend. There are advantages, however, in the use of the bend—a lessening in the resistance to the flow of the fluid in the pipe, a lessening in the number of fittings used, and, in many cases, a lessening in the cost of the material and erection. In some specifications copper bends are called for, and if the pipe is under a practically constant temperature, these may be fairly satisfactory, even if the stress in the metal is quite large. If the stress is kept fairly well under the elastic limit, steel pipe is, however, as good as copper pipe, and if the elastic limit is nearly reached or exceeded, there is always danger of the breaking of the pipe.

If the pipe is arranged as shown in Fig. 27, and if the expansion is the same on both sides of the loop, the center of the loop will act as a fixed point, and l_1 will be the length of one side of the loop. If now the expansion were altogether on one side of the loop, the entire length of both sides may be taken for l_1, which would appear to make it advantageous to place the loop near an anchorage instead of half way between two anchorages. The reason for this is that by placing the loop near one anchorage, we double the expansion acting on one side of the loop, but we also double l_1, and the expansion permissible varies as l_1^2 and not as l_1. If, however, the loop is placed half way between the two anchorages, the greatest movement of the pipe at any point is reduced by one-half, and, at times, this may be of considerable importance.

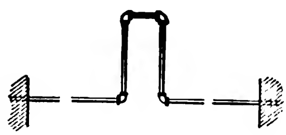

FIG. 27.

The method of using several small pipes or bends in place of one large pipe or bend has been suggested, and can sometimes be used to advantage. The intention is, of course, to make this section of piping capable of greater bending, and, at the same time, not to reduce the

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area through which the steam or fluid passes. The combined cross-sectional area of the small pipes may be made equal or larger than that of the large pipe, while the bending will be equal to that of one small pipe.

There is one point in connection with the expansion of pipes, to which particular attention is called. It is possible to reduce greatly the effects of expansion by the use of what is called a cold strain. William J. Baldwin, M. Am. Soc. C. E., was the first, the writer believes, to make use of cold strain in order to reduce the necessary allowance for expansion, and he has gone as far as to put the entire strain in the cold pipe in some cases.

By cold strain is meant the cutting of the pipe in such a manner that the pipe will be strained when cold in exactly the opposite way from that in which it is strained by expansion when hot. If the cold strain is 50% of the normal expansion, the pipe will be strained 50% in what may be called a negative direction when cold, and 50% in the opposite or positive direction when hot. By this means it is possible to reduce the strain in the pipe to half the normal strain of expansion, and thus reduce the necessary allowance for the same in this proportion. There are advantages, however, in making the cold strain greater than 50 per cent. If, for instance, the cold strain is exactly equal to the expansion, then the pipe is under strain when cold and entirely free from strain when hot or, in other words, the tendency to strain open one side of a pipe flange is a maximum when the pipe is cold or when there is no pressure on it, and it becomes zero when the pipe is hot or when there is a maximum pressure on it. The strain of expansion is also eliminated when that due to steam pressure is a maximum, so that the pipe is not subjected to the two strains at the same time.

If the cold strain is something less than the full expansion, these effects are, of course, proportionately decreased.

Perhaps it might be of interest to mention one instance in which the advantage of cold strain was used to remedy what seemed to be a serious trouble. A leak in one of the main steam pipes in a large hotel was causing much annoyance. The cause of the trouble was simply that the flanges were thrown out of line by the strain of the expansion of the piping. The location was such that it was of the utmost importance not to shut off the steam from this pipe for more

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than a very short time. Various expedients had been suggested, all requiring a shut-down of the plant for a considerable length of time, when it was suggested by Mr. Baldwin, that one length of flanged pipe be replaced by a similar piece of pipe, cut enough shorter, however, to eliminate entirely the excessive strain of expansion when the piping was hot. This new piece of pipe was made ready before the plant was shut down, and the time, therefore, during which it was not in operation, was limited to a very few minutes.

There is another advantage due to cold strain, which may be mentioned, and which will be appreciated particularly by the steamfitter or the man in charge of the erection of the work, wherever flanged piping is used. The advantage is simply this, that a flange joint when unbolted will tend to open up and allow an easy removal of a gasket.

In conclusion, a word may be said in regard to the use of the diagrams. They are calculated for a maximum fiber stress of 12 000 lb. per sq. in. This gives an ample factor of safety for wrought-iron or steel pipe, and more, perhaps, than some will wish to allow. It is very easy, however, to use the diagrams with a higher stress, if so desired, since the stress varies directly as the amount of expansion. If it is desired, for instance, to use a maximum fiber stress of 16 000 lb. per sq. in., it is only necessary to increase the amounts of expansion in the diagrams in the ratio of 16 000 to 12 000, or $\frac{4}{3}$. An expansion of 3 in. in the diagrams can then be made 4 in., and other values can be increased proportionately.

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DISCUSSION.

WILLIAM D. ENNIS, ESQ.* (by letter).—Mr. Taggart has rendered a real service in reducing to quantitative form the various assumptions which engineers have had to make in designing pipe lines in order to provide for expansion. There is no question as to the benefit, in some cases, of merely treating a long transmission line as suggested in Fig. 27; but this seems to be a clumsy expedient at best, to be resorted to only as a means of getting around a difficulty quickly and cheaply. The use of cold strain in erection has been thoroughly tried out by the fitters, and always with success; it cannot be too strongly insisted on that all piping should be erected in this way. Mr. Ennis.

The writer does not follow Mr. Taggart's apparent condemnation of the bend as an expedient for expansion resistance, having supposed that its shape, ordinarily at least, gives it a susceptibility to flexure greater than that of the straight pipe with elbows. Certainly there is less likelihood of leakage at the end joints when bends are used. If this is not due to greater flexibility, on what grounds is it to be explained?

Both cold straining and expansion strains have an important relation to flange pressure. The ordinary pipe flange, with a continuous face, has a contact pressure due to bolting only slightly in excess of that necessary to hold it against high strain pressures. Unless the question of anchorage is carefully worked out, cases sometimes arise in which the cold strain or the expansion may compel a flange to leak.

WILLIAM KENT, ESQ. (by letter).—It seems to the writer that the theory of the action of the pipes shown in Fig. 2 is not correct. Why should the pipe, *b d,* take the reverse curve there shown? If both pipes expand, and the elbow, *a,* moves to *b,* they would probably curve in a single direction, as shown in Fig. 28, tending to bend the right-angled elbow into an acute angle, or, if that is too stiff, to open the joints of the flanges if they are separated by gaskets, or to crack the flanges if they are metal to metal. If screwed elbows are used, the screwed ends of the pipes, being their weakest part, would tend to be distorted. Mr. Kent.

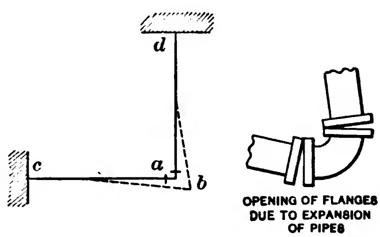

FIG. 28.

All the sketches seem to relate to practice which is no longer considered good, except for small pipes and low pressures. Fifteen or twenty years ago the expansion joint shown in Fig. 27 was not uncommon, but many accidents resulted from it, not only from

* Professor of Mechanical Engineering, Polytechnic Institute of Brooklyn.

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Mr. Kent. the cracking of the flanges due to expansion, but also from water-hammer. When steam pressures began to be 120 lb. and upward, and high-speed engines became common, accidents to steam pipes became most serious and frequent, and led to the adoption of long steel bends and forged-steel flanges, with no cast-iron or screwed joints. Steam-pipe lines, formerly the most dangerous part of a steam plant (accidents to them being more numerous than boiler explosions or bursting fly-wheels), have now become most reliable, an accident to a well-constructed modern pipe line being very rare.

What is needed for the aid of designers is a series of experiments on long pipe bends like those in Fig. 29, in order to ascertain how much expansion will be safely taken care of by their flexibility.

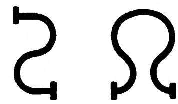

FIG. 29.

Referring to Mr. Taggart's criticism of taking care of expansion by swinging fittings, such fittings have sometimes been used with excellent results. A noted instance is that of the steam-pipe line of the Waldorf-Astoria Hotel, in which the pipe expansion was taken care of by right-angled bends and by loops like that shown in Fig. 27, the result being scores of cracked flanges, leaky screwed joints, and the like. The whole piping system was condemned as highly dangerous by several engineers who examined it, and it was relaid with swinging fittings, a few years after the hotel was built, with complete success, the writer has been informed. Expansion joints which have two pipe legs, connected with a sort of ball joint, are now on the market, and are giving good service, even with superheated steam at high pressure.

Mr. Taggart. RALPH C. TAGGART, ASSOC. M. AM. SOC. C. E. (by letter).—Mr. Ennis and Mr. Kent have both drawn one conclusion from the paper which certainly was not intended, namely, that the writer is opposed to the use of pipe bends. The writer is not opposed to pipe bends; in fact, he is a strong advocate of their use in many cases. In the paper, however, he desired to make clear the fact that pipe bends do not add to the flexibility of piping because of their shape. They often add to the possible flexibility of the piping, however, by the elimination of fittings, where the latter may be a source of weakness. This question was dealt with at some length in connection with Fig. 25, and, for this reason, the writer did not go into the question of the weakness of fittings when discussing pipe bends.

Any arrangement which will remove a fitting from a point of maximum strain is desirable. The location of these points was discussed in connection with Fig. 25. Such points, however, are not always located where the change of direction occurs.

The writer uses the right-angle diagrams because of their simplicity, and because the curves derived therefrom may be used, whether elbows or bends are installed.

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A careful analysis of the stresses and strains in a bent pipe, considered as an elastic arch, will show that the radius of the bend required for a fixed maximum fiber stress is practically the same as the length of one side of a square corner, where the turn in the pipe is as strong as the pipe itself. If the ends of the bends are held rigidly, both as to alignment and position, the analysis will show that the bend with a radius, *r*, will be more rigid than the square corner of a length, *l*, on each side, where *l* equals *r*. This is an unusual condition, however, and the bend may practically be calculated as if the pipe ran to a square corner. Mr. Taggart.

In regard to experiments on pipe bends which have been made in the past, attention is called to one fact, namely, that, where these tests are carried to a point at which the pipe ruptures, the pipe usually swings out of its original plane and bends in at least two planes. This results in greater bending in the pipe, before it breaks, than is shown by the calculations made on the basis of bending in a single plane; but when it is desired to keep the maximum fiber stress in the pipe down to what is generally considered to be good practice in steel structures, practically all the expansion occurs within one plane, and this is often essential on account of the alignment of the piping. The writer wishes to bring out the fact that, if the stresses are calculated from experiment, based on the point at or near which the pipe ruptures (bending in two planes), the stresses for lesser expansions, figured therefrom, will be too high, where now the pipe bends only within one plane. With this fact in mind, the writer believes that experiments on the expansion of pipes confirm the data in the paper.

Mr. Kent states that the theory of the action of the pipes under expansion shown in Fig. 2 does not seem to him to be correct. He asks: "Why should the pipe, *b d*, take the reverse curve there shown?" He suggests that, with expansion in two directions, the pipe would probably take the position shown in Fig. 28.

The reason for the pipes taking the reverse curve, and for the condition shown in Fig. 28 being incorrect, is simply that the flanges must not be allowed to open as shown in that figure. It is ordinarily an impossible condition, if the pipe is to carry steam. The flanges must be made to come together, and this tightening-up of the bolts on the outer sides of the flanges is what must necessarily put the reverse curve in the pipe.

If in any special case the condition cited by Mr. Kent occurs, the resulting strains may still be found from the diagrams given. The condition is simply one in which the bending moment at the fitting becomes zero. This is the same as the condition in which l_2 equals ∞, and is shown by the curves which are thus marked.

The condition shown in Fig. 2 is for an expansion in one direction only, in which case the maximum strain does not come at the elbow.

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Mr. Taggart. That shown in Fig. 28 is for an equal expansion in two directions, in which case the maximum strain comes at the elbow. The actual position of the pipe under the conditions represented in Fig. 28 would be that shown in Fig. 30.

Mr. Kent seems to think that the writer's sketches do not refer to and cannot be used in connection with bends. In this he is mistaken. The curves which Mr. Kent shows in Fig. 29 can be calculated by the diagrams in the paper. The writer would determine their

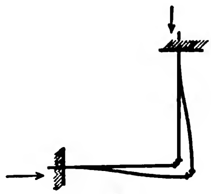

Fig. 30.

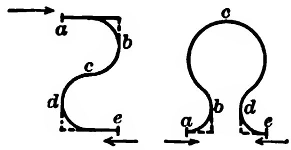

Fig. 31.

flexibility by calculating for a straightened pipe with 90° turns from each end of the bend to a point 90° from the direction of expansion. The remainder of the pipe is calculated directly on the basis of its length. For example, the bends shown in Fig. 29 are shown in Fig. 31.

The dotted lines from *a* to *b* and from *d* to *e* show how the pipe should be figured up to the points *b* and *d,* if the expansion is in the direction of the pipe at the end of the bends, or as shown by the arrows. The pipe between *b-c-d* can be calculated directly on the basis of its length, and as if it were at right angles to the direction of the expansion. If the expansion is at right angles to the direction of the arrows, the dotted lines would not be considered, and the entire length of the curved pipe would, of course, be calculated as if it were at right angles to the expansion.

Mr. Kent seems to think that the writer's sketches refer only to low-pressure work. This is not true. As before stated, they may be applied to bends and to piping as arranged for the highest pressures.

A great deal of piping is installed to-day with pressures of more than 120 lb. per sq. in., where elbows are used, which give satisfactory service. For the higher pressures, cast-steel, and not cast-iron, elbows are used.

The lessening, in recent years, in accidents from steam piping under high pressure, has been due largely to the more general use of steel in place of cast iron. Cast iron should never be used in piping under very high pressure because of its uncertainty. Water hammer will also injure cast iron much sooner than steel on account of its brittleness. Water hammer in steam piping, however, is a crime. Steel pipe and bends in high-pressure piping are of advantage, but if a serious water hammer occurs it is almost sure to cause trouble.

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In regard to expansion allowances, it may be said that the designer who has had experience may turn out a very satisfactory equipment, but in too many cases the factor of safety is too low. It is almost beyond the range of human possibility for experience alone to maintain a uniform factor of safety, and exact calculations should always be made where there is the slightest doubt as to the amount of allowance for expansion. Mr. Taggart.

In regard to the use of swinging fittings where allowance must be made for pipe expansion, the writer does not agree with Mr. Kent. He does not see how any unprejudiced person can reach a different conclusion from that contained in his paper, and knows that practically all operating engineers and steam-fitters of experience will condemn such joints. The only argument that he has ever heard in their favor is the statement that they have been used somewhere with success. Mr. Kent cites one instance. The writer went to the building mentioned, where the engineer described the old arrangement of the piping. It appears that the original installation contained standard-weight pipe, although the plant itself is a high-pressure steam plant. There was trouble, not only at the expansion loop, but at practically every other joint in the piping. The expansion loop itself was entirely too small to do any appreciable good. New high-pressure piping, all extra heavy, was installed. In the new arrangement the piping was run so as to allow for a swinging joint. Whether there is a movement at this joint, however, is unknown, as a constant steam pressure is maintained practically at all times. The engineer told the writer that he did not believe there was any movement within the thread of the fitting, and the writer's own observation leads him to believe that statement, the expansion being carried largely to the ends of the piping.

If, however, there is in any single case or in any number of cases a movement in the thread of the fitting without leakage, this certainly is the exception and not the rule. In the majority of cases, where there is no trouble with what are thought to be swinging joints it will be found that there is no movement in the thread, although such may be imagined.

In regard to the use of ball-and-socket joints as swinging expansion joints, the writer will only quote from a statement made by a company which has claimed in the past that it is the only successful all-metal flexible ball-joint manufacturer in the United States. The company states, "Will guarantee the joint if used constantly, unless you move the joint exactly in the same place for a long time." This condition appears to be the one that must be met in most cases where such a joint would be used as an expansion joint.

In connection with pipe expansion, there is a method of shortening expansion loops which is not mentioned in the paper. It consists in the use of standard-weight pipe for the bends or expansion loops, while

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Mr. Taggart. the remainder of the piping is extra heavy. An extra heavy pipe will bend as many inches as a lighter pipe of the same outside diameter, but it takes more force to produce this bending. The use of the lighter pipe bends, therefore, throws less strain on the other piping. It is much better to use longer loops, however, and maintain the extra heavy piping throughout.

In conclusion, a word should be added in regard to what were termed primary and secondary expansions. An approximate method of determining the primary or principal expansion was given in the paper. Usually, this is only a matter of simple observation; but, when there is any question as to which expansion is the primary and which is the secondary, it is a simple matter to calculate the piping both ways and take the values which show the greater strains.

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AMERICAN SOCIETY OF CIVIL ENGINEERS
INSTITUTED 1852

TRANSACTIONS

Paper No. 1168

TESTS OF CREOSOTED TIMBER.

By W. B. Gregory, M. Am. Soc. C. E.

During the last few years a quantity of literature has appeared in which the treatment of timber by preservatives has been discussed. The properties of timber, both treated and untreated, have been determined by the Forest Service, United States Department of Agriculture, and through its researches valuable knowledge has come to engineers who have to deal with the design of wooden structures. There is very little information, however, regarding the effect of time on creosoted timber, and for this reason the results given herewith may prove of interest.

The material tested consisted of southern pine stringers having a cross-section approximately 6 by 16 in. and a length of 30 ft. For the purpose of testing, each beam was cut into two parts, each about 15 ft. long. This material had been in use in a trestle of a railroad near New Orleans for 26 years. The stringers were chosen at random to determine the general condition of the trestle. The timber had been exposed to the weather and subjected to heavy train service from the time it was treated until it was tested. The annual rainfall at New Orleans is about 60 in., and the humidity of the air is high. In spite of these conditions, there was no appearance of decay on any of the specimens tested. The specifications under which the timber was treated were as follows:

Timber.

The timber for creosoting shall be long-leafed or southern pine. Sap surfaces on two or more sides are preferred.

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Piles.—The piles shall be of long-leafed or southern pine, not less than 14 in. at the butt. They shall be free from defects impairing their strength, and shall be reasonably straight.

The piles shall be cleanly peeled, no inner skin being left on them. The oil used shall be so-called creosote oil, from London, England, and shall be of a heavy quality.

The treatment will vary according to the dimensions of the timbers and length of time they have been cut. Timbers of large and small dimensions shall not be treated in the same charge, neither shall timbers of differing stages of air seasoning, or the close-grained, be treated in the same charge with coarse or open-grained timbers.

The timbers shall be subjected first to live steam superheated to from 250 to 275° Fahr., and under a 30 to 40-lb. pressure. The live steam shall be admitted into the cylinders through perforated steam pipes, and the temperature shall be obtained by using superheated steam in closed pipes in the cylinders.

The length of time this steaming shall last will depend on the size of the timbers and the length of time they have been cut. In piles and large timbers freshly cut, as long a time as 12 hours may be required. After the steaming is accomplished, the live steam shall be shut off and the superheated steam shall be maintained at a temperature of 160° or more and a vacuum of from 20 to 25 in. shall be held for 4 hours or longer, if the discharge from the pumps indicates the necessity.

Oil Treatment.—The temperature being maintained at 160° Fahr., the cylinders shall be promptly filled with creosote oil at a temperature as high as practicable (about 100° Fahr.). The oil shall be maintained at a pressure ranging from 100 to 120 lb., as experience and measurements must determine the length of time the oil treatment shall continue, so that the required amount of oil may be injected.

After the required amount of oil is injected, the superheated steam shall be shut off, the oil let out, the cylinders promptly opened at each end, and the timber immediately removed from the cylinder.

In the erection of timbers the sap side must be turned up, and framing or cutting of timbers shall not be permitted, if avoidable. All cut surfaces of timbers shall be saturated with hot asphaltum, thinned with creosote oil. The heads of piles when cut shall be promptly coated with the hot asphaltum and oil, even though the cut-off be temporary.

Method of Testing.

The tests were made on a Riehlé 100 000-lb. machine in the Experimental Engineering Laboratory of Tulane University of Louisiana. The machine is provided with a cast-iron beam for cross-bending tests. The distance between supports was 12 ft. The method of support was

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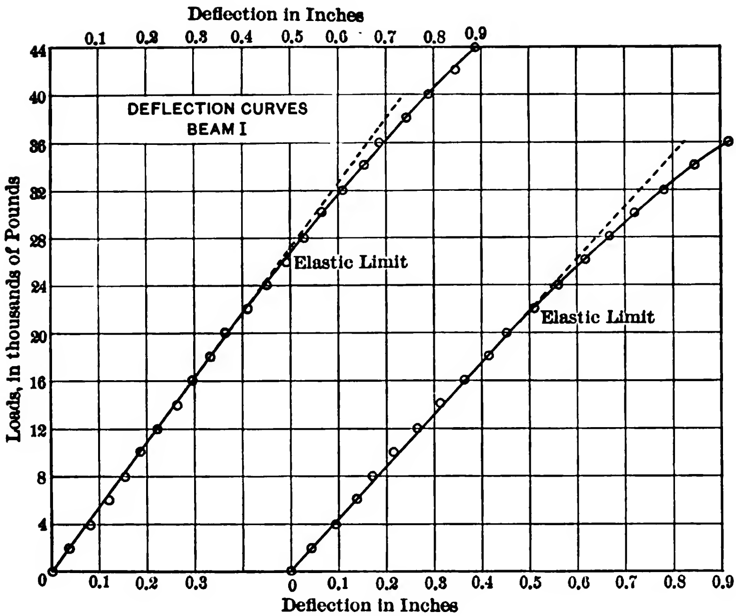

FIG. 1.

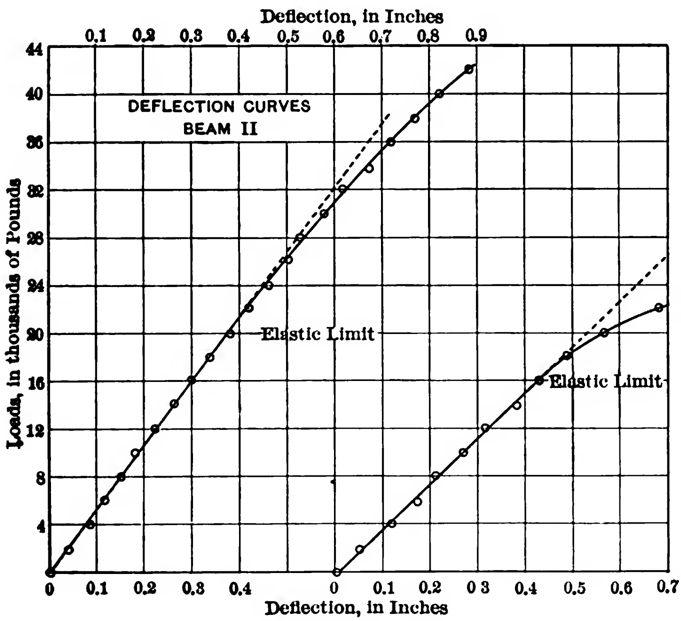

FIG. 2.

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as follows: Each end of the beam was provided with a steel roller which rested on the cast-iron beam of the testing machine, while above the roller, and, directly under the beam tested, there was a steel plate 6 by 8 in. in area and 1 in. thick. The area was sufficiently great to distribute the load and prevent the shearing of the fibers of the wood. The head of the Riehlé machine is 10 in. wide. A plate, ⅜ in. thick, 6 in. wide and 18 in. long, was placed between the head of the machine and the beam tested.

TABLE 1.—Summary of Results of Transverse Tests of Beams at Tulane University, February 10th to March 2d, 1909.

Number of beam.	Top or butt of log.	b Width, in inches.	h Height, in inches.	I $I = \frac{b h^3}{12}$	Loads: Actual at elastic limit.	Loads: Maximum.	$S = \frac{P l c}{4 I}$ At elastic limit.	$S = \frac{P l c}{4 I}$ Maximum.	d, Inches. At elastic limit.	E $E = \frac{P l^3}{48 d I}$	Weight, in pounds per cubic foot.	Remarks.
I	B	6.28	15.94	2 120	22 000	45 900	2 975	6 200	0.41	1 575 000	50.2	Close-grained pine,
I	T	6.00	15.69	1 934	20 000	38 000	2 915	5 540	0.465	1 888 000	47.5	long-leaf.
II*	T	6.37	15.81	2 098	20 000	43 450	2 722	5 918	0.380	1 562 000	40.5	Coarse loblolly,
II	B	6.41	16.41	2 360	16 000	23 040	1 999	8 180	0.430	979 000	42.2	large knots.
III	T	5.88	15.63	1 871	24 000	45 180	3 608	6 785	0.535	1 489 000	40.4	Close-grained,
III	B	5.88	15.90	1 965	21 000	35 190	3 054	5 120	0.515	1 288 000	44.2	long-leaf, no knots.
IV	T	6.00	15.48	1 835	22 000	38 425	3 320	5 810	0.465	1 601 000	40.8	Loblolly, with
IV	B	6.12	15.37	2 032	22 000	35 500	3 090	4 983	0.660	1 017 000	41.5	knots.
V	B	6.00	16.00	2 048	22 000	47 000	3 090	6 610	0.400	1 670 000	47.2	Long-leaf yellow
V*	T	6.00	15.87	1 999	14 000	22 050	1 998	3 145	0.315	1 382 000	42.1	pine.
VI*	B	5.50	15.75	1 790	22 000	51 380	3 484	8 925	0.450	1 695 000	50.2	Long-leaf yellow
VI*	T	5.87	15.62	1 865	20 000	44 000	3 018	6 627	0.410	1 625 000	45.2	pine.
VII	B	6.56	15.62	2 083	34 000	51 900	4 580	6 985	0.620	1 637 000	43.7	Long-leaf yellow
VII*	T	6.22	15.62	1 975	20 000	49 000	2 845	6 970	0.380	1 658 000	40.2	pine.

* Failed in longitudinal shear.

The deflection was measured on both sides of each beam by using silk threads stretched on each side from nails driven about 2 in. above the bottom of the beam and directly over the rollers which formed the supports. From a small piece of wood, tacked to the bottom of the beam at its center and projecting at the sides, the distance to these threads was measured. These measurements were taken to the nearest hundredth of an inch. The mean of the deflections was taken as the true deflection for any load.

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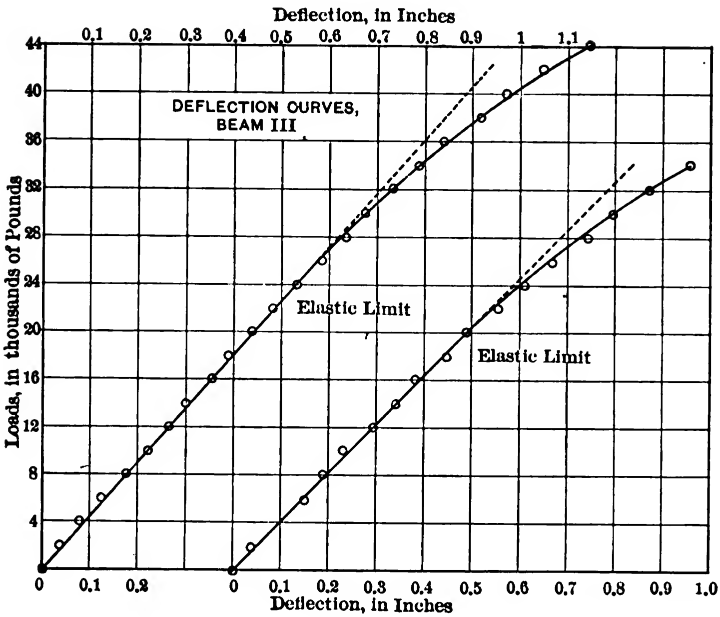
Deflection, in Inches
0.1
0.2
0.3
0.4
0.5
0.6
0.7
0.8
0.9
1
1.1
44
40
36
32
28
24
20
16
12
8
4
Loads, in thousands of Pounds
DEFLECTION CURVES, BEAM III
Elastic Limit
Elastic Limit
0
0.1
0.2
0
0.1
0.2
0.3
0.4
0.5
0.6
0.7
0.8
0.9
1.0
Deflection, in Inches

FIG. 3.

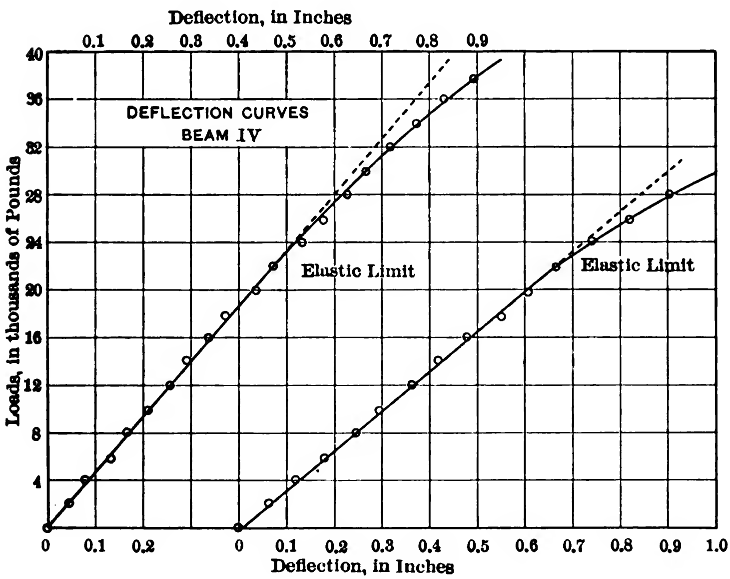
Deflection, in Inches
0.1
0.2
0.3
0.4
0.5
0.6
0.7
0.8
0.9
40
36
32
28
24
20
16
12
8
4
Loads, in thousands of Pounds
DEFLECTION CURVES
BEAM IV
Elastic Limit
Elastic Limit
0
0.1
0.2
0
0.1
0.2
0.3
0.4
0.5
0.6
0.7
0.8
0.9
1.0
Deflection, in Inches

FIG. 4.

In computing the various quantities shown in Table 1, the summary of results, the load has been assumed as concentrated at the center of the beam. While it is true that the load was spread over a length of about 12 in., due to the width of the head of the machine and the plate between it and the beam tested, it is also true that there were irregularities, such as bolt-holes and, in some cases, abrasions due to wear, that could not well be taken into account. Hence, it was deemed sufficiently accurate to consider the load as concentrated. Besides the horizontal bolt-holes, shown in the photographs, there were vertical bolt-holes, at intervals in all the beams. The latter were ⅞ in. in diameter, and in every case they were sufficiently removed from the center of the length of the beam to allow the maximum moment at the reduced section to be relatively less than that at the center of the beam. For this reason, no correction was made for these holes. The broken beams often showed that rupture started at, or was influenced by, some of the holes, especially the horizontal ones.

While some of the heavy oils of a tarry consistency remained, they were only to be found in the sappy portions of the long-leaf pine and in the loblolly (Specimens II and IV). Exposure in a semi-tropical climate for 26 years had resulted in the removal of the more volatile portions of the creosote oil. The penetration of the oil into the sap wood seemed to be perfect, while in the loblolly it varied from a fraction of an inch to 1½ in. In the heart wood there was very little penetration across the grain. The timber had been framed and the holes bored before treatment. The penetration of the creosote along the grain from the holes was often from 4 to 6 in.

Circular 39 of the Forest Service, U. S. Department of Agriculture, entitled "Experiments on the Strength of Treated Timber," gives the results of a great many tests of creosoted ties, principally loblolly pine, from which the following conclusions are quoted:

"(1) A high degree of steaming is injurious to wood. The degree of steaming at which pronounced harm results will depend upon the quality of the wood and its degree of seasoning, and upon the pressure (temperature) of steam and the duration of its application. For loblolly pine the limit of safety is certainly 30 pounds for 4 hours, or 20 pounds for 6 hours." [Tables 3, 6, and 7.]

"(2) The presence of zinc chlorid will not weaken wood under static loading, although the indications are that the wood becomes brittle under impact." [Tables 3 and 4.]

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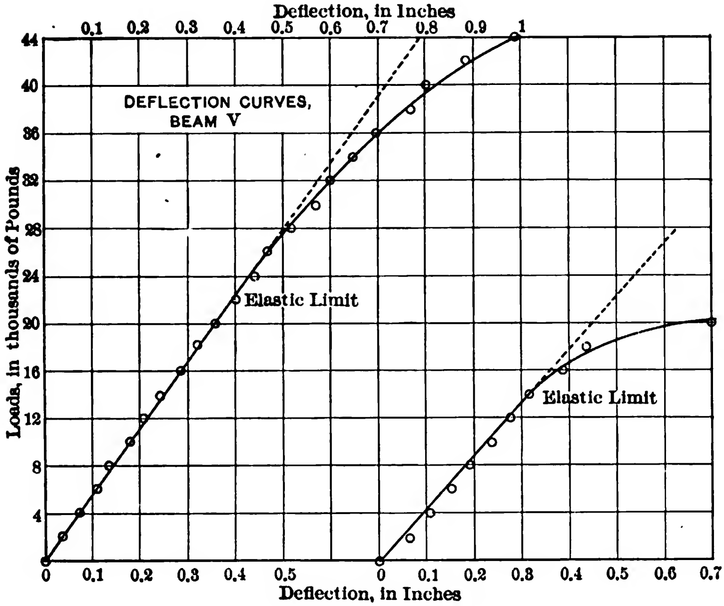
Deflection, in Inches
0.1 0.2 0.3 0.4 0.5 0.6 0.7 0.8 0.9 1
DEFLECTION CURVES,
BEAM V
Loads, in thousands of Pounds
44 40 36 32 28 24 20 16 12 8 4
Elastic Limit
Elastic Limit
0 0.1 0.2 0.3 0.4 0.5 0 0.1 0.2 0.3 0.4 0.5 0.6 0.7
Deflection, in Inches

FIG. 5.

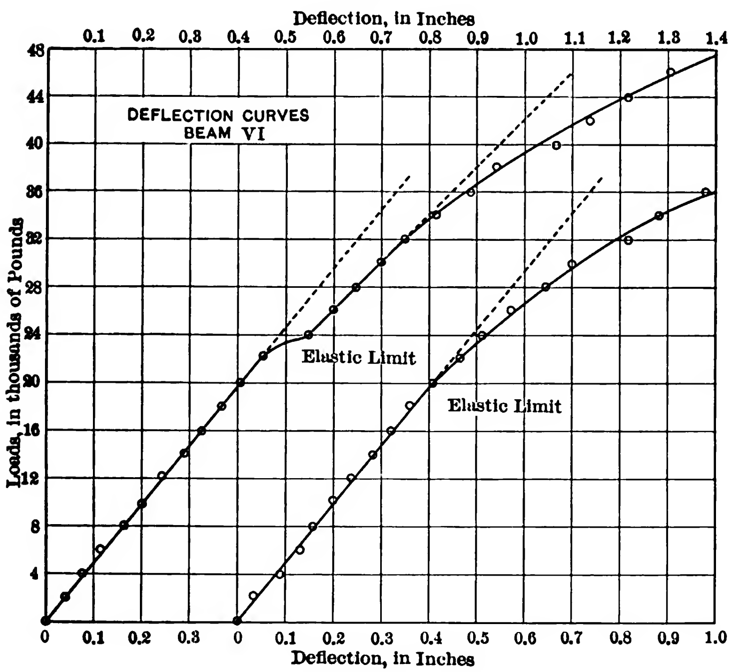
Deflection, in Inches
0.1 0.2 0.3 0.4 0.5 0.6 0.7 0.8 0.9 1.0 1.1 1.2 1.3 1.4
DEFLECTION CURVES
BEAM VI
Loads, in thousands of Pounds
48 44 40 36 32 28 24 20 16 12 8 4
Elastic Limit
Elastic Limit
0 0.1 0.2 0.3 0 0.1 0.2 0.3 0.4 0.5 0.6 0.7 0.8 0.9 1.0
Deflection, in Inches

FIG. 6.

"(3) The presence of creosote will not weaken wood of itself. Since apparently it is present only in the openings of the cells, and does not get into the cell walls, its action can only be to retard the seasoning of the wood." [Tables 3, 4, 5, and 6.]

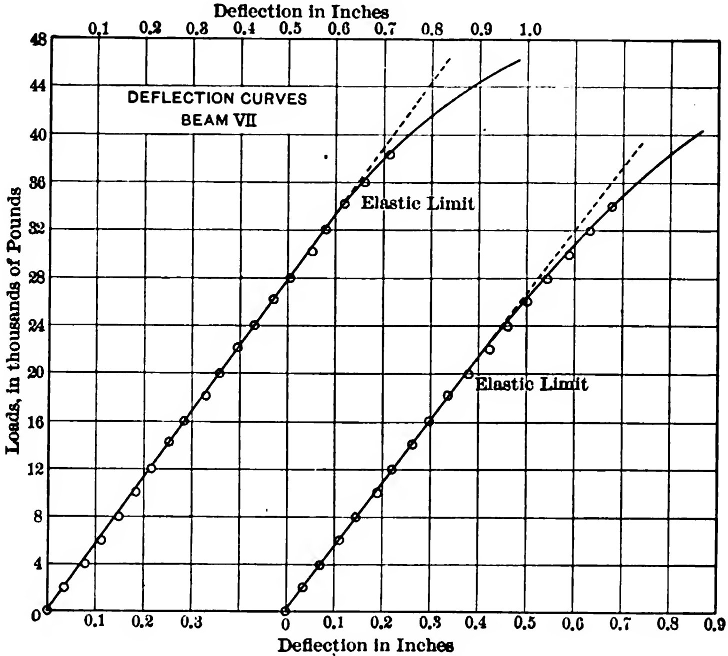

FIG. 7.

COMPARISONS.

A comparison of the results obtained with tests made on untreated timber is interesting, and to this end Tables 2 and 3, from Circular 115, Forest Service, U. S. Department of Agriculture, by W. Kendrick Hatt, Assoc. M. Am. Soc. C. E., are quoted. The tests made by the writer were from timber raised in Louisiana and Mississippi, while the tests quoted were from timber raised farther north. The number of tests was not sufficient to settle questions of average strength or other qualities. It will be seen, however, that the treated timber 26 years old compares favorably with the new untreated timber.

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PLATE I.
TRANS. AM. SOC. CIV. ENGRS.
VOL. LXX, No. 1168.
GREGORY ON
TESTS OF CREOSOTED TIMBER.

FIG. 1.—SPECIMEN IN TESTING MACHINE, SHOWING METHOD OF SUPPORT.

FIG. 2.—END VIEWS OF TESTED TIMBERS.

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TABLE 2.—BENDING STRENGTH OF LARGE STICKS.

LOBLOLLY PINE.

Reference number.	Locality of Growth.	Dimensions. Section, in inches.	Dimensions. Span, in feet.	Grade.	Condition of seasoning.		Number of tests.	Moisture, per cent.	Rings per inch.	Specific gravity, dry.	Weight per Cubic Foot, in Pounds. As tested.	Weight per Cubic Foot, in Pounds. Oven dry.	Fiber stress at elastic limit, in pounds per square inch.	Modulus of rupture, in pounds per square inch.	Modulus of elasticity, in thousands of pounds per square inch.	Elastic resilience, in inch-pounds per cubic inch.	Number failing by longitudinal shear.	Remarks.
1	South Carolina.	6 by 7 6 by 10 4 by 12 6 by 16 8 by 14 8 by 16	10 to 15.5	Square edge....	Green ...	Average...	42	48.0	5.7	0.50	46.2	31.2	3 150	5 580	1 426	0.45	7	Moisture above saturation point in all cases.
						Maximum.		92.1	11.7	0.60	56.8	37.5	5 210	8 460	1 920	0.99		
						Minimum..		30.2	2.3	0.40	35.6	25.0	1 675	3 120	905	0.07		
2	South Carolina.	6 by 7 4 by 12 6 by 10 6 by 16 8 by 16 10 by 16	10 to 16	Square edge....	Partially air dry.	Average...	18	27.7	5.0	0.50	40.0	31.2	3 380	5 650	1 485	0.45	0	Moisture from 25 to 30 per cent.
						Maximum.		29.2	8.2	0.55	43.7	34.4	4 610	8 090	1 880	0.76		
						Minimum..		25.5	2.5	0.45	35.6	28.1	2 115	3 600	1 152	0.20		
3	South Carolina.	6 by 7 4 by 12 6 by 10 6 by 16	10 to 15	Square edge....	Partially air dry.	Average...	19	21.0	5.6	0.50	37.5	31.2	2 970	5 690	1 340	0.39	2	Moisture less than 25 per cent.
						Maximum.		24.9	17.2	0.58	45.6	36.2	4 850	8 100	2 040	0.69		
						Minimum..		15.0	2.7	0.41	31.2	25.6	1 730	2 910	906	0.10		
4	Virginia...	8 by 8	6 to 16	Square edge....	Partially air dry.	Average...	12	22.4	4.8	0.46	35.6	28.8	3 260	5 180	1 180	0.51	0	
						Maximum.		27.7	8.8	0.58	43.1	36.2	5 300	8 950	1 728	1.05		
						Minimum.		17.8	2.5	0.37	30.0	23.1	1 280	2 180	606	0.13		
5	Virginia..	8 by 8	6 to 15.5	Square edge....	Green....	Average..	17	64.0	3.0	0.43	43.7	26.9	1 985	3 490	744	0.31	0	Very rapid growth; poor quality.
						Maximum.		100.5	4.0	0.51	51.9	31.9	3 185	4 720	1 193	0.78		
						Minimum.		38.8	2.5	0.35	35.0	21.9	956	2 180	357	0.12		

LONG-LEAF PINE.

Reference number.	Locality of Growth.	Section, in inches.	Span, in feet.	Grade.	Condition of seasoning.		Number of tests.	Moisture, per cent.	Rings per inch.	Specific gravity, dry.	As tested.	Oven dry.	Fiber stress at elastic limit.	Modulus of rupture.	Modulus of elasticity.	Elastic resilience.	Number failing by longitudinal shear.	Remarks.
6	South Carolina.	6 by 8 10 by 16	15	Merchantable......	Partially air dry.	Average...	22	25.0	13.7	0.58	45.6	36.2	3 800	7 160	1 560	0.58	9	
						Maximum.		40.8	25.4	0.76	60.0	47.5	4 970	10 080	2 010	0.78		
						Minimum..		17.8	6.2	0.50	39.4	31.2	2 220	5 450	1 190	0.21		
7	Georgia....	10 by 12	15	Merchantable......	Partially air dry.	Average..	22	27.3	18.0	0.69	54.7	42.9	5 581	8 384	1 690	......	6	Excellent merchantable grade.
						Maximum.		34.5	29.0	0.79	.....	49.4	9 600	11 410	2 920	......		
						Minimum..		20.0	11.0	0.50	.....	31.4	3 547	4 886	1 167	......		

TABLE 3.—LOBLOLLY PINE.—BENDING TESTS ON BEAMS SEASONED UNDER DIFFERENT CONDITIONS.

(8 by 16-in. section ; 13½ to 15-ft. span.)

	Number of tests.	Fiber stress at elastic limit, in pounds per square inch.	Modulus of rupture, in pounds per square inch.	Longitudinal shear at maximum load, in pounds per square inch.	Modulus of elasticity, in thousands of pounds per square inch.	Percentage of moisture.	Rings per inch.	Weight per cubic foot, oven dry, in pounds.	Condition of seasoning.
Average.......	4	3 580	5 480	364_4	1 780	23.2	9.4	33.7	Air dry, 3½ months in the open.
Maximum.....		4 070	6 600	440	1 987	24.3	11.5	34.5	
Minimum......		3 090	5 000	327	1 530	21.5	8.0	32.5	
Average.......	5	4 512	5 060	333_3	1 685	20	7.7	33.9	Kiln dry, 6 days.
Maximum.....		5 840	7 320	488	1 790	22	10.2	38.0	
Minimum......		3 180	2 150	143	1 410	18	4.7	27.7	
Average.......	12	4 331	6 721	493_9	1 688	......	7.7	......	Air dry, 21 months under shelter.
Maximum.....		4 990	8 560	620	2 002	......	9.5	......	
Minimum......		3 110	5 160	380	1 398	......	5.5	......	

NOTE.—Figures written as subscripts to the figures for longitudinal shear indicate the number of sticks failing in that manner.

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SIDE VIEWS OF TESTED TIMBERS.

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TABLE 4.—Load and Deflection Log. Beam I.

Date: February 26th, 1909.
$l = 12$ ft.; b (mean) $= 6\frac{9}{32}$ in.;
h (mean) $= 15\frac{15}{16}$ in.;
$c = 7.97$ in. Time = 1 hour.

Date: February 24th, 1909.
$l = 12$ ft.; b (mean) $= 6$ in.;
h (mean) $= 15.69$ in.;
$c = 7.84$ in.

No.	P	Deflection, in Inches.					P	Deflection, in Inches.				
	Load, in pounds.	Reading.	Total deflection.	Reading.	Total deflection.	Mean total deflection.	Load, in pounds.	Reading.	Total deflection.	Reading.	Total deflection.	Mean total deflection.
1	0	1.86	0	1.88	0	0	0	1.83	0	1.86	0	0
2	2 000	1.92	0.05	1.90	0.02	0.035	2 000	1.87	0.04	1.90	0.04	0.04
3	4 000	1.96	0.10	1.94	0.06	0.080	4 000	1.91	0.08	1.96	0.10	0.090
4	6 000	1.99	0.13	1.98	0.10	0.115	6 000	1.96	0.13	2.00	0.14	0.135
5	8 000	2.03	0.17	2.02	0.14	0.155	8 000	2.00	0.17	2.04	0.18	0.175
6	10 000	2.05	0.19	2.06	0.18	0.185	10 000	2.04	0.21	2.08	0.22	0.215
7	12 000	2.10	0.24	2.09	0.21	0.225	12 000	2.09	0.26	2.13	0.27	0.265
8	14 000	2.13	0.27	2.13	0.25	0.260	14 000	2.14	0.31	2.18	0.32	0.315
9	16 000	2.17	0.31	2.16	0.28	0.295	16 000	2.19	0.36	2.23	0.37	0.365
10	18 000	2.20	0.34	2.20	0.32	0.330	18 000	2.24	0.41	2.28	0.42	0.415
11	20 000	2.24	0.36	2.25	0.37	0.365	20 000	2.29	0.46	2.33	0.47	0.465
12	22 000	2.28	0.42	2.28	0.40	0.410	22 000	2.34	0.51	2.39	0.53	0.520
13	24 000	2.32	0.46	2.32	0.44	0.450	24 000	2.39	0.56	2.43	0.57	0.565
14	26 000	2.36	0.50	2.36	0.48	0.490	26 000	2.44	0.61	2.48	0.62	0.615
15	28 000	2.40	0.54	2.39	0.51	0.525	28 000	2.49	0.66	2.53	0.67	0.665
16	30 000	2.43	0.57	2.44	0.56	0.565	30 000	2.55	0.72	2.58	0.72	0.720
17	32 000	2.48	0.62	2.48	0.60	0.610	32 000	2.61	0.78	2.65	0.79	0.785
18	34 000	2.52	0.66	2.53	0.65	0.655	34 000*	2.68	0.85	2.70	0.84	0.845
19	36 000	2.56	0.70	2.56	0.68	0.690	36 000	2.74	.91	2.78	0.92	0.915
20	38 000	2.61	0.75	2.62	0.74	0.745	38 000			Broke.		
21	40 000	2.65	0.79	2.67	0.79	0.790						
22	42 000	2.70	0.84	2.73	0.85	0.845						
23	44 000	2.75	0.89	2.77	0.89	0.890						

37 500 lb., First Crack; 45 900 lb., Failed.

At Elastic Limit: Load, 22 000 lb.; deflection, 0.41 in.; S, 2 975 lb.
Maximum: Load, 45 900 lb.; deflection, ...; S, 6 209 lb.
$E = 1\,575\,000$ lb.

At Elastic Limit: Load, 20 000 lb.; deflection, 0.465 in.; S, 2 975 lb.
Maximum: Load, 38 000 lb.; deflection, ...; S, 5 540 lb.
$E = 1\,383\,000$ lb.

* First crack.

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TABLE 4.—(*Continued.*)—LOAD AND DEFLECTION LOG. BEAM II.

Date: February 20th, 1909.

$l = 12$ ft.; b (mean) $= 6.38$ in.; h (mean) $= 15.81$ in.; $c = 7.91$ in. Time $= 47.5$ min.

No.	*P* Load, in pounds.	Deflection, in Inches. Reading.	Total deflection.	Reading.	Total deflection.	Mean total deflection.
1	0	1.65	0	1.68	0	0
2	2 000	1.69	0.04	1.72	0.04	0.040
3	4 000	1.73	0.08	1.77	0.09	0.085
4	6 000	1.76	0.11	1.80	0.12	0.115
5	8 000	1.80	0.15	1.83	0.15	0.150
6	10 000	1.83	0.18	1.86	0.18	0.180
7	12 000	1.87	0.22	1.90	0.22	0.220
8	14 000	1.91	0.26	1.94	0.26	0.260
9	16 000	1.95	0.30	1.98	0.30	0.300
10	18 000	1.98	0.33	2.02	0.34	0.335
11	20 000	2.03	0.38	2.06	0.38	0.380
12	22 000	2.07	0.42	2.10	0.42	0.420
13	24 000	2.11	0.46	2.14	0.46	0.460
14	26 000	2.15	0.50	2.18	0.50	0.500
15	28 000	2.18	0.53	2.22	0.54	0.535
16	30 000	2.23	0.58	2.26	0.58	0.580
17	32 000	2.27	0.62	2.30	0.62	0.620
18	34 000	2.32	0.67	2.35	0.67	0.670
19	36 000	2.37	0.72	2.40	0.72	0.720
20	38 000	2.42	0.77	2.45	0.77	0.770
21	40 000	2.48	0.83	2.50	0.82	0.825
22	42 000	2.53	0.88	2.56	0.88	0.880
23	43 450			Fracture.		
24	45 710			Failed.		

At Elastic Limit: Load, 20 000 lb.; deflection, 0.38 in.; S, 2 722 lb.
Maximum: Load, 43 450 lb.; deflection, ...; S, 5 918 lb.
$E = 1\ 562\ 000$ lb.

Date: ....

$l = 12$ ft.; b (mean) $= 6.41$ in.; h (mean) $= 16.41$ in.; $c = 8.20$ in.

No.	*P* Load, in pounds.	Deflection, in Inches. Reading.	Total deflection.	Reading.	Total deflection.	Mean total deflection.
1	0	1.86	0	1.87	0	0
2	2 000	1.91	0.05	1.92	0.05	0.05
3	4 000	1.98	0.12	1.98	0.11	0.115
4	6 000	2.05	0.19	2.02	0.15	0.170
5	8 000	2.07	0.21	2.08	0.21	0.210
6	10 000	2.13	0.27	2.13	0.26	0.265
7	12 000	2.18	0.32	2.18	0.31	0.315
8	14 000	2.25	0.39	2.24	0.37	0.380
9	16 000	2.30	0.44	2.29	0.42	0.430
10	18 000*	2.35	0.49	2.35	0.48	0.485
11	20 000	2.44	0.58	2.42	0.55	0.565
12	22 000	2.54	0.68	2.54	0.67	0.675
13	25 040			Failed.		

At Elastic Limit: Load, 16 000 lb.; deflection, 0.43 in.; S, 1 999 lb.
Maximum: Load, 25 040 lb.; deflection, ...; S, 3 130 lb.
$E = 979\ 000$ lb.

* First crack.

TABLE 4.—(*Continued.*)—LOAD AND DEFLECTION LOG. BEAM III.

Date: February 13th, 1909.	Date: ....
l = 12 ft.; b (mean) = 5.88 in.; h (mean) = 15.63 in.; c = 7.82 in.	l = 12 ft.; b (mean) = 5.88 in.; h (mean) = 15.9 in.; c = 7.95 in. Time = 45 min.

No.	*P*	Deflection, in Inches.					*P*	Deflection, in Inches.				
	Load, in pounds.	Reading.	Total deflection.	Reading.	Total deflection.	Mean total deflection.	Load, in pounds.	Reading.	Total deflection.	Reading.	Total deflection.	Mean total deflection.
1	0	1.23	0	1.06	0	0	0	1.67	0	1.63	0	0
2	2 000	1.27	0.04	1.10	0.04	0.040	2 000	1.70	0.03	1.68	0.05	0.040
3	4 000	1.32	0.09	1.13	0.07	0.080	4 000	1.72	0.05	1.72	0.09	0.070
4	6 000	1.37	0.14	1.17	0.11	0.125	6 000	1.82	0.15	1.78	0.15	0.150
5	8 000	1.42	0.19	1.22	0.16	0.175	8 000	1.86	0.19	1.82	0.19	0.190
6	10 000	1.47	0.24	1.26	0.20	0.220	10 000	1.90	0.23	1.87	0.24	0.235
7	12 000	1.51	0.28	1.31	0.25	0.265	12 000	1.97	0.30	1.92	0.29	0.295
8	14 000	1.55	0.32	1.35	0.29	0.305	14 000	2.00	0.33	1.98	0.35	0.340
9	16 000	1.60	0.37	1.40	0.34	0.355	16 000	2.03	0.36	2.04	0.41	0.385
10	18 000	1.64	0.41	1.44	0.38	0.395	18 000	2.10	0.43	2.09	0.46	0.445
11	20 000	1.68	0.45	1.49	0.43	0.440	20 000	2.13	0.46	2.14	0.51	0.485
12	22 000	1.72	0.49	1.54	0.48	0.485	22 000	2.20	0.53	2.20	0.57	0.550
13	24 000	1.73	0.55	1.58	0.52	0.535	24 000	2.26	0.59	2.26	0.63	0.610
14	26 000	1.82	0.59	1.64	0.58	0.585	26 000	2.31	0.64	2.32	0.69	0.665
15	28 000	1.88	0.65	1.68	0.62	0.635	28 000	2.38	0.71	2.40	0.77	0.740
16	30 000	1.92	0.69	1.73	0.67	0.680	30 000	2.42	0.75	2.47	0.84	0.795
17	32 000	1.97	0.74	1.79	0.78	0.735	32 000	2.49	0.82	2.55	0.92	0.870
18	34 000	2.02	0.79	1.85	0.79	0.790	34 000	2.58	0.91	2.62	0.99	0.950
19	36 000	2.07	0.84	1.90	0.84	0.840						
20	38 000	2.13	0.90	1.97	0.91	0.915						
21	40 000	2.20	0.97	2.03	0.97	0.970						
22	42 000	2.27	1.04	2.11	1.05	1.045						
23	44 000	2.37	1.14	2.21	1.15	1.145						

39 100 lb. First Crack; 45 180 lb. Failed.

At Elastic Limit: Load, 24 000 lb.; deflection, 0.535 in.; S, 3 608 lb.
Maximum: Load, 45 180 lb.; deflection, ...; S, 6 785 lb.
E = 1 489 000 lb.

22 000 lb. First Crack; 35 190 lb. Failed.

At Elastic Limit: Load, 21 000 lb.; deflection, 0.515 in.; S, 3 054 lb.
Maximum: Load, 35 190 lb.; deflection, ...; S, 5 120 lb.
E = 1 288 000 lb.

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TABLE 4.—(*Continued.*)—LOAD AND DEFLECTION LOG. BEAM IV.

Date: February 16th, 1909.
l = 12 ft.; b (mean) = 6.0 in.; h (mean) = 15.43 in.; c = 7.71 in.

Date: February 10th, 1909.
l = 12 ft.; b (mean) = 6.12 in.; h (mean) = 15.87 in.; c = 7.93 in. Time = 30 min.

No.	P	Deflection, in Inches.					P	Deflection, in Inches.				
	Load, in pounds.	Reading.	Total deflection.	Reading.	Total deflection.	Mean total deflection.	Load, in pounds.	Reading.	Total deflection.	Reading.	Total deflection.	Mean total deflection.
1	0	2.28	0	2.05	0	0	0	1.44	0	1.58	0	0
2	2 000	2.31	0.03	2.10	0.05	0.040	2 000	1.50	0.06	1.64	0.06	0.06
3	4 000	2.34	0.06	2.14	0.09	0.075	4 000	1.55	0.11	1.70	0.12	0.115
4	6 000	2.40	0.12	2.19	0.14	0.130	6 000	1.62	0.18	1.76	0.18	0.180
5	8 000	2.43	0.15	2.23	0.18	0.165	8 000	1.68	0.24	1.82	0.24	0.240
6	10 000	2.47	0.19	2.28	0.23	0.210	10 000	1.72	0.28	1.89	0.31	0.295
7	12 000	2.51	0.23	2.32	0.27	0.250	12 000	1.80	0.36	1.94	0.36	0.360
8	14 000	2.54	0.26	2.37	0.32	0.290	14 000	1.85	0.41	2.00	0.42	0.415
9	16 000	2.59	0.31	2.41	0.36	0.335	16 000	1.90	0.46	2.06	0.48	0.470
10	18 000	2.62	0.34	2.45	0.40	0.370	18 000	1.98	0.54	2.13	0.55	0.545
11	20 000	2.68	0.40	2.50	0.45	0.425	20 000	2.03	0.59	2.19	0.61	0.600
12	22 000	2.72	0.44	2.54	0.49	0.465	22 000	2.09	0.65	2.25	0.67	0.660
13	24 000	2.78	0.50	2.60	0.55	0.525	24 000	2.15	0.71	2.33	0.75	0.730
14	26 000	2.82	0.54	2.65	0.60	0.570	26 000	2.23	0.79	2.42	0.84	0.815
15	28 000	2.87	0.59	2.69	0.64	0.615	28 000	2.32	0.88	2.49	0.91	0.895
16	30 000	2.91	0.63	2.74	0.69	0.660	30 000	2.42	0.98	2.62	1.04	1.010
17	32 000	2.97	0.69	2.78	0.73	0.710	32 000	2.56	1.12	2.74	1.16	1.140
18	34 000	3.01	0.73	2.85	0.80	0.765	34 000	2.67	1.23	2.87	1.29	1.265
19	36 000	3.07	0.79	2.90	0.85	0.820						
20	38 000	3.14	0.86	2.98	0.93	0.895						

34 000 lb. First Crack; 38 425 lb. Failed.

At Elastic Limit: Load, 22 000 lb.; deflection, 0.465 in.; S, 3 320 lb.
Maximum: Load. 38 425 lb.; deflection, ...; S, 5 810 lb.
E = 1 601 000 lb.

28 360 lb. Cracked; 35 500 lb. Failed.

At Elastic Limit: Load, 22 000 lb.; deflection, 0.66 in.; S, 3 090 lb.
Maximum: Load. 35 500 lb.; deflection, ...; S, 4 983 lb.
E = 1 017 000 lb.

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TABLE 4.—(*Continued.*)—Load and Deflection Log. Beam V.

Date:	**Date: February 27th, 1909.**
$l = 12$ ft.; b (mean) $= 6$ in.; h (mean) $= 16$ in. $c = 8$ in. Time $= 40$ min.	$l = 12$ ft.; b (mean) $= 6$ in.; h (mean) $= 15.87$ in.; $c = 7.94$ in.

No.	*P* Load, in pounds.	Deflection, in Inches. Reading.	Total deflection.	Reading.	Total deflection.	Mean total deflection.	*P* Load, in pounds.	Deflection, in Inches. Reading.	Total deflection.	Reading.	Total deflection.	Mean total deflection.
1	0	1.97	0	1.37	0	0	0	1.31	0	1.25	0	0
2	2 000	2.01	0.04	1.40	0.03	0.035	2 000	1.37	0.06	1.31	0.06	0.06
3	4 000	2.06	0.09	1.43	0.06	0.075	4 000	1.41	0.10	1.36	0.11	0.105
4	6 000	2.08	0.11	1.47	0.10	0.105	6 000	1.46	0.15	1.40	0.15	0.150
5	8 000	2.11	0.14	1.50	0.13	0.135	8 000	1.49	0.18	1.45	0.20	0.190
6	10 000	2.16	0.19	1.54	0.17	0.180	10 000	1.54	0.23	1.49	0.24	0.235
7	12 000	2.19	0.22	1.57	0.20	0.210	12 000	1.58	0.27	1.53	0.28	0.275
8	14 000	2.22	0.25	1.61	0.24	0.245	14 000	1.62	0.31	1.57	0.32	0.315
9	16 000	2.25	0.28	1.65	0.28	0.280	16 000	1.68	0.37	1.65	0.40	0.385
10	18 000	2.29	0.32	1.69	0.32	0.320	18 000	1.73	0.41	1.71	0.46	0.435
11	20 000	2.32	0.35	1.73	0.36	0.355	20 000	1.99	0.68	1.97	0.72	0.700
12	22 000	2.36	0.39	1.78	0.41	0.400						
13	24 000	2.39	0.42	1.83	0.46	0.440						
14	26 000	2.42	0.45	1.85	0.48	0.465						
15	28 000	2.47	0.50	1.90	0.53	0.515						
16	30 000	2.50	0.53	1.95	0.58	0.565						
17	32 000	2.54	0.57	1.99	0.62	0.595						
18	34 000	2.59	0.62	2.04	0.67	0.645						
19	36 000	2.63	0.66	2.09	0.72	0.690						
20	38 000	2.68	0.71	2.17	0.80	0.755						
21	40 000	2.73	0.76	2.21	0.84	0.800						
22	42 000	2.80	0.83	2.30	0.93	0.880						
23	44 000	2.90	0.93	2.40	1.03	0.980						

25 000 lb. Slight Crack; 47 000 lb. Failed.	20 000 lb. First Crack; 22 050 lb. Failed.
At Elastic Limit: Load, 22 000 lb.; deflection, 0.40 in.; *S*, 3 090 lb.	At Elastic Limit: Load, 14 000 lb.; deflection, 0.315 in.; *S*, 1 998 lb.
Maximum: Load, 47 000 lb.; deflection. ......; *S*, 6 610 lb.	Maximum: Load, 22 050 lb.; deflection, ...; *S*, 3 145 lb.
$E = 1\ 670\ 000$ lb.	$E = 1\ 382\ 000$ lb.

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TABLE 4.—(*Continued.*)—LOAD AND DEFLECTION LOG. BEAM VI.

Date: February 12th, 1909.	Date: February 13th, 1909.
$l = 12$ ft.; b (mean) $= 5.5$ i h (mean) $= 15.75$ in.; c $= 7.88$ in. Time $= 40$ min.	$l = 12$ ft.; b (mean) $= 5.87$ in.; h (mean) $= 15.62$ in.; c $= 7.81$ in.

No.	*P*	DEFLECTION, IN INCHES.					*P*	DEFLECTION, IN INCHES.				
	Load, in pounds.	Reading.	Total deflection.	Reading.	Total deflection.	Mean total deflection.	Load, in pounds.	Reading.	Total deflection.	Reading.	Total deflection.	Mean total deflection.
1	0	1.22	0	1.30	0	0	0	1.28	0	1.30	0	0
2	2 000	1.26	0.04	1.34	0.04	0.04	2 000	1.30	0.02	1.35	0.05	0.035
3	4 000	1.29	0.07	1.38	0.08	0.075	4 000	1.36	0.08	1.39	0.09	0.085
4	6 000	1.33	0.11	1.42	0.12	0.115	6 000	1.40	0.12	1.44	0.14	0.130
5	8 000	1.37	0.15	1.47	0.17	0.160	8 000	1.43	0.15	1.47	0.17	0.160
6	10 000	1.42	0.20	1.51	0.21	0.205	10 000	1.47	0.19	1.51	0.21	0.200
7	12 000	1.45	0.23	1.55	0.25	0.240	12 000	1.51	0.23	1.56	0.26	0.245
8	14 000	1.50	0.28	1.59	0.29	0.285	14 000	1.55	0.27	1.60	0.30	0.285
9	16 000	1.54	0.32	1.63	0.33	0.325	16 000	1.59	0.31	1.64	0.34	0.325
10	18 000	1.58	0.36	1.68	0.38	0.370	18 000	1.62	0.34	1.69	0.39	0.365
11	20 000	1.61	0.39	1.72	0.42	0.405	20 000	1.66	0.38	1.74	0.44	0.410
12	22 000	1.66	0.44	1.76	0.46	0.450	22 000	1.71	0.43	1.80	0.50	0.465
13	24 000	1.81	0.59	1.81	0.51	0.550	24 000	1.77	0.49	1.84	0.54	0.515
14	26 000	1.86	0.64	1.86	0.56	0.600	26 000	1.83	0.55	1.90	0.60	0.575
15	28 000	1.91	0.69	1.91	0.61	0.650	28 000	1.90	0.62	1.97	0.67	0.645
16	30 000	1.96	0.74	1.96	0.66	0.700	30 000	1.97	0.69	2.02	0.72	0.705
17	32 000	2.00	0.78	2.02	0.72	0.750	32 000	2.12	0.84	2.10	0.80	0.820
18	34 000	2.04	0.82	2.11	0.81	0.815	34 000	2.20	0.92	2.16	0.86	0.885
19	36 000	2.10	0.88	2.20	0.90	0.890	36 000	2.29	1.01	2.24	0.94	0.975
20	38 000	2.16	0.94	2.25	0.95	0.945	38 000	2.39	1.11	2.32	1.02	1.065
21	40 000	2.28	1.06	2.38	1.08	1.070						
22	42 000	2.38	1.16	2.42	1.12	1.140						
23	44 000	2.44	1.22	2.52	1.22	1.220						
24	46 000	2.53	1.31	2.60	1.30	1.305						
25	48 000	2.66	1.44	2.71	1.41	1.425						
26	50 000	2.78	1.56	2.87	1.57	1.565						

February 12th, 1909	February 13th, 1909
33 000 lb. First Crack; 51 330 lb. Failed.	24 000 lb., First Crack; 44 000 lb., Failed.
At Elastic Limit: Load, 22 000 lb.; deflection, 0.45 in.; *S*, 3 484 lb.	At Elastic Limit: Load, 20 000 lb.; deflection, 0.41 in.; *S*, 3 013 lb.
Maximum: Load, 51 330 lb.; deflection. ......; *S*, 8 925 lb.	Maximum: Load, 44 000 lb.; deflection, ...; *S*, 6 627 lb.
E=1 695 000 lb.	*E* =1 625 000 lb.

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TABLE 4.—(*Continued.*)—LOAD AND DEFLECTION LOG. BEAM VII.

	Date: March 2d, 1909. l = 12 ft.; b (mean) = 6.56 in.; h (mean) = 15.62 in.; c = 7.81 in. Time = 1 hr.						Date: February 20th, 1909. l = 12 ft.; b (mean) = 6.22 in.; h (mean) = 15.62 in.; c = 7.81 in. Time = 33 min.					
	P	DEFLECTION, IN INCHES.					*P*	DEFLECTION, IN INCHES.				
No.	Load, in pounds.	Reading.	Total deflection.	Reading.	Total deflection.	Mean total deflection.	Load, in pounds.	Reading.	Total deflection.	Reading.	Total deflection.	Mean total deflection.
1	0	1.84	0	1.71	0	0	0	1.69	0	1.73	0	0
2	2 000	1.88	0.04	1.74	0.03	0.035	2 000	1.72	0.03	1.77	0.04	0.035
3	4 000	1.92	0.08	1.79	0.08	0.080	4 000	1.76	0.07	1.80	0.07	0.070
4	6 000	1.96	0.12	1.81	0.10	0.110	6 000	1.80	0.11	1.84	0.11	0.110
5	8 000	2.00	0.16	1.85	0.14	0.150	8 000	1.84	0.15	1.87	0.14	0.145
6	10 000	2.03	0.19	1.89	0.18	0.185	10 000	1.88	0 19	1.92	0.19	0.190
7	12 000	2.06	0.22	1.93	0.22	0.220	12 000	1.91	0.22	1.95	0.22	0.220
8	14 000	2.11	0.27	1.95	0.24	0.255	14 000	1.95	0.26	2.00	0.27	0.265
9	16 000	2.14	0.30	1.99	0.28	0.290	16 600	1.99	0.30	2.03	0.30	0.300
10	18 000	2.18	0.34	2.03	0.32	0.330	18 000	2.03	0.34	2.06	0.33	0.335
11	20 000	2.22	0.38	2.05	0.34	0.360	20 000	2.07	0.38	2.11	0.38	0.380
12	22 000	2.25	0.41	2.10	0.39	0.400	22 000	2.11	0.42	2.16	0.43	0.425
13	24 000	2.29	0.45	2.13	0.42	0.435	24 000	2.15	0.46	2.20	0.47	0.465
14	26 000	2.32	0.48	2.17	0.46	0.470	26 000	2.19	0.50	2.24	0.51	0.505
15	28 000	2.36	0.52	2.21	0.50	0.510	28 000	2.23	0.54	2.28	0.55	0.545
16	30 000	2.40	0.56	2.25	0.54	0.550	30 000	2.27	0.58	2.33	0.60	0.590
17	32 000	2.43	0.59	2.29	0.58	0.585	32 000	2.32	0.63	2.37	0.64	0.635
18	34 000	2.47	0.63	2.32	0.61	0.620	34 000	2.36	0.67	2.42	0.69	0.680
19	36 000	2.51	0.67	2.37	0.66	0.665	36 000					
20	38 000	2.56	0.72	2.41	0.70	0.710						

27 000 lb., First Crack; 51 900 lb., Failed.

At Elastic Limit: Load, 34 000 lb.; deflection, 0.62 in.; S, 4 580 lb.
Maximum: Load, 51 900 lb.; deflection, ...; S, 6 985 lb.
E = 1 637 000 lb.

28 000 lb., First Crack; 49 000 lb., Failed.

At Elastic Limit: Load, 20 000 lb.; deflection, 0.38 in.; S, 2 845 lb.
Maximum: Load, 49 000 lb.; deflection, ...; S, 6 970 lb.
E = 1 658 000 lb.

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AMERICAN SOCIETY OF CIVIL ENGINEERS
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TRANSACTIONS

Paper No. 1169

SOME MOOTED QUESTIONS IN REINFORCED CONCRETE DESIGN.*

By Edward Godfrey, M. Am. Soc. C. E.

With Discussion by Messrs. Joseph Wright, S. Bent Russell, J. R. Worcester, L. J. Mensch, Walter W. Clifford, J. C. Meem, George H. Myers, Edwin Thacher, C. A. P. Turner, Paul Chapman, E. P. Goodrich, Albin H. Beyer, John C. Ostrup, Harry F. Porter, John Stephen Sewell, Sanford E. Thompson, and Edward Godfrey.

Not many years ago physicians had certain rules and practices by which they were guided as to when and where to bleed a patient in order to relieve or cure him. What of those rules and practices to-day? If they were logical, why have they been abandoned?

It is the purpose of this paper to show that reinforced concrete engineers have certain rules and practices which are no more logical than those governing the blood-letting of former days. If the writer fails in this, by reason of the more weighty arguments on the other side of the questions he propounds, he will at least have brought out good reasons which will stand the test of logic for the rules and practices which he proposes to condemn, and which, at the present time, are quite lacking in the voluminous literature on this comparatively new subject.

Destructive criticism has recently been decried in an editorial in an engineering journal. Some kinds of destructive criticism are of the highest benefit; when it succeeds in destroying error, it is reconstructive. No reform was ever accomplished without it, and no

* Presented at the meeting of March 16th, 1910.

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reformer ever existed who was not a destructive critic. If showing up errors and faults is destructive criticism, we cannot have too much of it; in fact, we cannot advance without it. If engineering practice is to be purged of its inconsistencies and absurdities, it will never be done by dwelling on its excellencies.

Reinforced concrete engineering has fairly leaped into prominence and apparently into full growth, but it still wears some of its swaddling-bands. Some of the garments which it borrowed from sister forms of construction in its short infancy still cling to it, and, while these were, perhaps, the best makeshifts under the circumstances, they fit badly and should be discarded. It is some of these misfits and absurdities which the writer would like to bring prominently before the Engineering Profession.

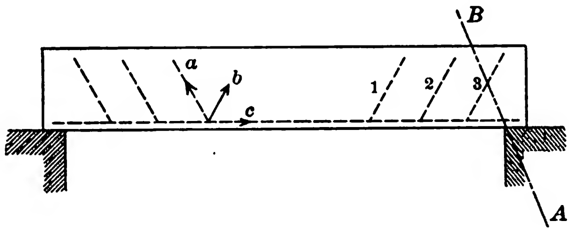

FIG. 1.

The first point to which attention is called, is illustrated in Fig. 1. It concerns sharp bends in reinforcing rods in concrete. Fig. 1 shows a reinforced concrete design, one held out, in nearly all books on the subject, as a model. The reinforcing rod is bent up at a sharp angle, and then may or may not be bent again and run parallel with the top of the beam. At the bend is a condition which resembles that of a hog-chain or truss-rod around a queen-post. The reinforcing rod is the hog-chain or the truss-rod. Where is the queen-post? Suppose this rod has a section of 1 sq. in. and an inclination of 60° with the horizontal, and that its unit stress is 16 000 lb. per sq. in. The forces, *a* and *b*, are then 16 000 lb. The force, *c*, must be also 16 000 lb. What is to take this force, *c*, of 16 000 lb.? There is nothing but concrete. At 500 lb. per sq. in., this force would require an area of 32 sq. in. Will some advocate of this type of design please state where this area can be found? It must, of necessity, be in contact with the rod, and, for structural reasons, because of the lack of stiffness in the rod, it would have to be close to the point of bend. If analogy to the queen-

post fails so completely, because of the almost complete absence of the post, why should not this borrowed garment be discarded?

If this same rod be given a gentle curve of a radius twenty or thirty times the diameter of the rod, the side unit pressure will be from one-twentieth to one-thirtieth of the unit stress on the steel. This being the case, and being a simple principle of mechanics which ought to be thoroughly understood, it is astounding that engineers should perpetrate the gross error of making a sharp bend in a reinforcing rod under stress.

The second point to which attention is called may also be illustrated by Fig. 1. The rod marked 3 is also like the truss-rod of a queen-post truss in appearance, because it ends over the support and has the same shape. But the analogy ends with appearance, for the function of a truss-rod in a queen-post truss is not performed by such a reinforcing rod in concrete, for other reasons than the absence of a post. The truss-rod receives its stress by a suitable connection at the end of the rod and over the support of the beam. The reinforcing rod, in this standard beam, ends abruptly at the very point where it is due to receive an important element of strength, an element which would add enormously to the strength and safety of many a beam, if it could be introduced.

Of course a reinforcing rod in a concrete beam receives its stress by increments imparted by the grip of the concrete; but these increments can only be imparted where the tendency of the concrete is to stretch. This tendency is greatest near the bottom of the beam, and when the rod is bent up to the top of the beam, it is taken out of the region where the concrete has the greatest tendency to stretch. The function of this rod, as reinforcement of the bottom flange of the beam, is interfered with by bending it up in this manner, as the beam is left without bottom-flange reinforcement, as far as that rod is concerned, from the point of bend to the support.

It is true that there is a shear or a diagonal tension in the beam, and the diagonal portion of the rod is apparently in a position to take this tension. This is just such a force as the truss-rod in a queen-post truss must take. Is this reinforcing rod equipped to perform this office? The beam is apt to fail in the line, *A B*. In fact, it is apt to crack from shrinkage on this or almost any other line, and to leave the strength dependent on the reinforcing steel. Suppose such a crack

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should occur. The entire strength of the beam would be dependent on the grip of the short end of Rod 3 to the right of the line, A B. The grip of this short piece of rod is so small and precarious, considering the important duty it has to perform, that it is astounding that designers, having any care for the permanence of their structures, should consider for an instant such features of design, much less incorporate them in a building in which life and property depend on them.

The third point to which attention is called, is the feature of design just mentioned in connection with the bent-up rod. It concerns the anchorage of rods by the embedment of a few inches of their length in concrete. This most flagrant violation of common sense has its most conspicuous example in large engineering works, where of all places better judgment should prevail. Many retaining walls have been built, and described in engineering journals, in papers before engineering societies of the highest order, and in books enjoying the greatest reputation, which have, as an essential feature, a great number of rods which cannot possibly develop their strength, and might as well be of much smaller dimensions. These rods are the vertical and horizontal rods in the counterfort of the retaining wall shown at *a*, in Fig. 2. This retaining wall consists of a front curtain wall and a horizontal slab joined at intervals by ribs or counterforts. The manifest and only function of the rib or counterfort is to tie together the curtain wall and the horizontal slab. That it is or should be of concrete is because the steel rods which it contains, need protection. It is clear that failure of the retaining wall could occur by rupture through the Section A B, or through B C. It is also clear that, apart from the cracking of the concrete of the rib, the only thing which would produce this rupture is the pulling out of the short ends of these reinforcing rods. Writers treat the triangle, A B C, as a beam, but there is absolutely no analogy between this triangle and a beam. Designers seem to think that these rods take the place of so-called shear rods in a beam, and that the inclined rods are equivalent to the rods in a tension flange of a beam. It is hard to understand by what process of reasoning such results can be attained. Any clear analysis leading to these conclusions would certainly be a valuable contribution to the literature on the subject. It is scarcely possible, however, that such analysis will be brought forward, for it is the apparent policy of

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the reinforced concrete analyst to jump into the middle of his proposition without the encumbrance of a premise.

There is positively no evading the fact that this wall could fail, as stated, by rupture along either *A B* or *B C*. It can be stated just as positively that a set of rods running from the front wall to the horizontal slab, and anchored into each in such a manner as would be adopted were these slabs suspended on the rods, is the only rational and the only efficient design possible. This design is illustrated at *b* in Fig. 2.

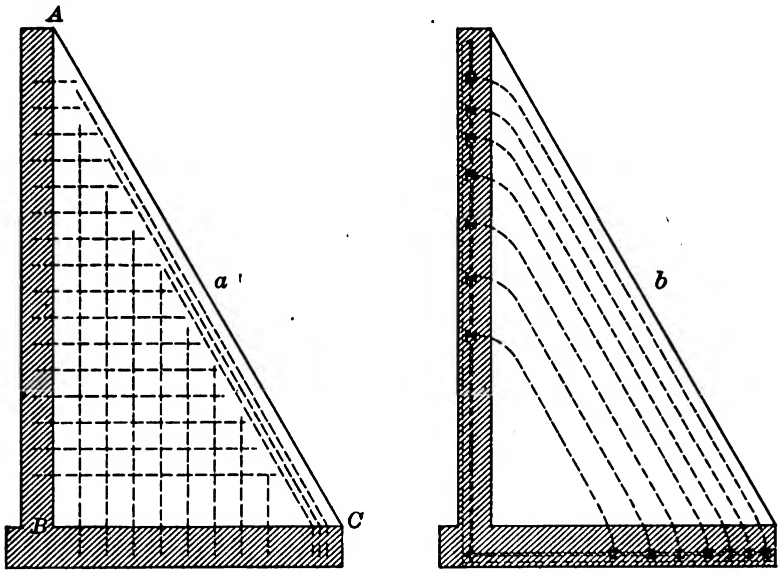

FIG. 2.

The fourth point concerns shear in steel rods embedded in concrete. For decades, specifications for steel bridges have gravely given a unit shear to be allowed on bridge pins, and every bridge engineer knows or ought to know that, if a bridge pin is properly proportioned for bending and bearing, there is no possibility of its being weak from shear. The centers of bearings cannot be brought close enough together to reduce the size of the pin to where its shear need be considered, because of the width required for bearing on the parts. Concrete is about one-thirtieth as strong as steel in bearing. There is, therefore, somewhat less than one-thirtieth of a reason for specifying any shear on steel rods embedded in concrete.

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The gravity of the situation is not so much the serious manner in which this unit of shear in steel is written in specifications and building codes for reinforced concrete work (it does not mean anything in specifications for steelwork, because it is ignored), but it is apparent when designers soberly use these absurd units, and proportion shear rods accordingly.

Many designers actually proportion shear rods for shear, shear in the steel at units of 10 000 or 12 000 lb. per sq. in.; and the blame for this dangerous practice can be laid directly to the literature on reinforced concrete. Shear rods are given as standard features in the design of reinforced concrete beams. In the Joint Report of the Committee of the various engineering societies, a method for proportioning shear members is given. The stress, or shear per shear member, is the longitudinal shear which would occur in the space from member to member. No hint is given as to whether these bars are in shear or tension; in fact, either would be absurd and impossible without greatly overstressing some other part. This is just a sample of the state of the literature on this important subject. Shear bars will be taken up more fully in subsequent paragraphs.

The fifth point concerns vertical stirrups in a beam. These stirrups are conspicuous features in the designs of reinforcing concrete beams. Explanations of how they act are conspicuous in the literature on reinforced concrete by its total absence. By stirrups are meant the so-called shear rods strung along a reinforcing rod. They are usually U-shaped and looped around the rod.

It is a common practice to count these stirrups in the shear, taking the horizontal shear in a beam. In a plate girder, the rivets connecting the flange to the web take the horizontal shear or the increment to the flange stress. Compare two ¾-in. rivets tightly driven into holes in a steel angle, with a loose vertical rod, ¾ in. in diameter, looped around a reinforcing rod in a concrete beam, and a correct comparison of methods of design in steel and reinforced concrete, as they are commonly practiced, is obtained.

These stirrups can take but little hold on the reinforcing rods—and this must be through the medium of the concrete—and they can take but little shear. Some writers, however, hold the opinion that the stirrups are in tension and not in shear, and some are bold enough to compare them with the vertical tension members of a Howe truss.

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Imagine a Howe truss with the vertical tension members looped around the bottom chord and run up to the top chord without any connection, or hooked over the top chord; then compare such a truss with one in which the end of the rod is upset and receives a nut and large washer bearing solidly against the chord. This gives a comparison of methods of design in wood and reinforced concrete, as they are commonly practiced.

Anchorage or grip in the concrete is all that can be counted on, in any event, to take up the tension of these stirrups, but it requires an embedment of from 30 to 50 diameters of a rod to develop its full strength. Take 30 to 50 diameters from the floating end of these shear members, and, in some cases, nothing or less than nothing will be left. In any case the point at which the shear member, or stirrup, is good for its full value, is far short of the centroid of compression of the beam, where it should be; in most cases it will be nearer the bottom of the beam. In a Howe truss, the vertical tension members having their end connections near the bottom chord, would be equivalent to these shear members.

The sixth point concerns the division of stress into shear members. Briefly stated, the common method is to assume each shear member as taking the horizontal shear occurring in the space from member to member. As already stated, this is absurd. If stirrups could take shear, this method would give the shear per stirrup, but even advocates of this method acknowledge that they can not. To apply the common analogy of a truss: each shear member would represent a tension web member in the truss, and each would have to take all the shear occurring in a section through it.

If, for example, shear members were spaced half the depth of a beam apart, each would take half the shear by the common method. If shear members take vertical shear, or if they take tension, what is between the two members to take the other half of the shear? There is nothing in the beam but concrete and the tension rod between the two shear members. If the concrete can take the shear, why use steel members? It is not conceivable that an engineer should seriously consider a tension rod in a reinforced concrete beam as carrying the shear from stirrup to stirrup.

The logical deduction from the proposition that shear rods take tension is that the tension rods must take shear, and that they must

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take the full shear of the beam, and not only a part of it. For these shear rods are looped around or attached to the tension rods, and since tension in the shear rods would logically be imparted through the medium of this attachment, there is no escaping the conclusion that a large vertical force (the shear of the beam) must pass through the tension rod. If the shear member really relieves the concrete of the shear, it must take it all. If, as would be allowable, the shear rods take but a part of the shear, leaving the concrete to take the remainder, that carried by the rods should not be divided again, as is recommended by the common method.

Bulletin No. 29 of the University of Illinois Experiment Station shows by numerous experiments, and reiterates again and again, that shear rods do not act until the beam has cracked and partly failed. This being the case, a shear rod is an illogical element of design. Any element of a structure, which cannot act until failure has started, is not a proper element of design. In a steel structure a bent plate which would straighten out under a small stress and then resist final rupture, would be a menace to the rigidity and stability of the structure. This is exactly analogous to shear rods which cannot act until failure has begun.

When the man who tears down by criticism fails to point out the way to build up, he is a destructive critic. If, under the circumstances, designing with shear rods had the virtue of being the best thing to do with the steel and concrete disposed in a beam, as far as experience and logic in their present state could decide, nothing would be gained by simply criticising this method of design. But logic and tests have shown a far simpler, more effective, and more economical means of disposing of the steel in a reinforced concrete beam.

In shallow beams there is little need of provision for taking shear by any other means than the concrete itself. The writer has seen a reinforced slab support a very heavy load by simple friction, for the slab was cracked close to the supports. In slabs, shear is seldom provided for in the steel reinforcement. It is only when beams begin to have a depth approximating one-tenth of the span that the shear in the concrete becomes excessive and provision is necessary in the steel reinforcement. Years ago, the writer recommended that, in such beams, some of the rods be curved up toward the ends of the span and anchored over the support. Such reinforcement completely

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relieves the concrete of all shearing stress, for the stress in the rod will have a vertical component equal to the shear. The concrete will rest in the rod as a saddle, and the rod will be like the cable of a suspension span. The concrete could be in separate blocks with vertical joints, and still the load would be carried safely.

By end anchorage is not meant an inch or two of embedment in concrete, for an iron vise would not hold a rod for its full value by such means. Neither does it mean a hook on the end of the rod. A threaded end with a bearing washer, and a nut and a lock-nut to hold the washer in place, is about the only effective means, and it is simple and cheap. Nothing is as good for this purpose as plain round rods, for no other shape affords the same simple and effective means of end connection. In a line of beams, end to end, the rods may be extended into the next beam, and there act to take the top-flange tension, while at the same time finding anchorage for the principal beam stress.

The simplicity of this design is shown still further by the absence of a large number of little pieces in a beam box, as these must be held in their proper places, and as they interfere with the pouring of the concrete.

It is surprising that this simple and unpatented method of design has not met with more favor and has scarcely been used, even in tests. Some time ago the writer was asked, by the head of an engineering department of a college, for some ideas for the students to work up for theses, and suggested that they test beams of this sort. He was met by the astounding and fatuous reply that such would not be reinforced concrete beams. They would certainly be concrete beams, and just as certainly be reinforced.

Bulletin 29 of the University of Illinois Experiment Station contains a record of tests of reinforced concrete beams of this sort. They failed by the crushing of the concrete or by failure in the steel rods, and nearly all the cracks were in the middle third of the beams, whereas beams rich in shear rods cracked principally in the end thirds, that is, in the neighborhood of the shear rods. The former failures are ideal, and are easier to provide against. A crack in a beam near the middle of the span is of little consequence, whereas one near the support is a menace to safety.

The seventh point of common practice to which attention is called, is the manner in which bending moments in so-called continuous beams

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are juggled to reduce them to what the designer would like to have them. This has come to be almost a matter of taste, and is done with as much precision or reason as geologists guess at the age of a fossil in millions of years.

If a line of continuous beams be loaded uniformly, the maximum moments are negative and are over the supports. Who ever heard of a line of beams in which the reinforcement over the supports was double that at mid-spans? The end support of such a line of beams cannot be said to be fixed, but is simply supported, hence the end beam would have a negative bending moment over next to the last support equal to that of a simple span. Who ever heard of a beam being reinforced for this? The common practice is to make a reduction in the bending moment, at the middle of the span, to about that of a line of continuous beams, regardless of the fact that they may not be continuous or even contiguous, and in spite of the fact that the loading of only one gives quite different results, and may give results approaching those of a simple beam.

If the beams be designed as simple beams—taking the clear distance between supports as the span and not the centers of bearings or the centers of supports—and if a reasonable top reinforcement be used over these supports to prevent cracks, every requirement of good engineering is met. Under extreme conditions such construction might be heavily stressed in the steel over the supports. It might even be overstressed in this steel, but what could happen? Not failure, for the beams are capable of carrying their load individually, and even if the rods over the supports were severed—a thing impossible because they cannot stretch out sufficiently—the beams would stand.

Continuous beam calculations have no place whatever in designing stringers of a steel bridge, though the end connections will often take a very large moment, and, if calculated as continuous, will be found to be strained to a very much larger moment. Who ever heard of a failure because of continuous beam action in the stringers of a bridge? Why cannot reinforced concrete engineering be placed on the same sound footing as structural steel engineering?

The eighth point concerns the spacing of rods in a reinforced concrete beam. It is common to see rods bunched in the bottom of such a beam with no regard whatever for the ability of the concrete to grip the steel, or to carry the horizontal shear incident to their stress, to

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the upper part of the beam. As an illustration of the logic and analysis applied in discussing the subject of reinforced concrete, one well-known authority, on the premise that the unit of adhesion to rod and of shear are equal, derives a rule for the spacing of rods. His reasoning is so false, and his rule is so far from being correct, that two-thirds would have to be added to the width of beam in order to make it correct. An error of 66% may seem trifling to some minds, where reinforced concrete is considered, but errors of one-tenth this amount in steel design would be cause for serious concern. It is reasoning of the most elementary kind, which shows that if shear and adhesion are equal, the width of a reinforced concrete beam should be equal to the sum of the peripheries of all reinforcing rods gripped by the concrete. The width of the beam is the measure of the shearing area above the rods, taking the horizontal shear to the top of the beam, and the peripheries of the rods are the measure of the gripping or adhesion area.

Analysis which examines a beam to determine whether or not there is sufficient concrete to grip the steel and to carry the shear, is about at the vanishing point in nearly all books on the subject. Such misleading analysis as that just cited is worse than nothing.

The ninth point concerns the T-beam. Excessively elaborate formulas are worked out for the T-beam, and haphazard guesses are made as to how much of the floor slab may be considered in the compression flange. If a fraction of this mental energy were directed toward a logical analysis of the shear and gripping value of the stem of the T-beam, it would be found that, when the stem is given its proper width, little, if any, of the floor slab will have to be counted in the compression flange, for the width of concrete which will grip the rods properly will take the compression incident to their stress.

The tenth point concerns elaborate theories and formulas for beams and slabs. Formulas are commonly given with 25 or 30 constants and variables to be estimated and guessed at, and are based on assumptions which are inaccurate and untrue. One of these assumptions is that the concrete is initially unstressed. This is quite out of reason, for the shrinkage of the concrete on hardening puts stress in both concrete and steel. One of the coefficients of the formulas is that of the elasticity of the concrete. No more variable property of concrete is known than its coefficient of elasticity, which may vary from 1 000 000

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to 5 000 000 or 6 000 000; it varies with the intensity of stress, with the kind of aggregate used, with the amount of water used in mixing, and with the atmospheric condition during setting. The unknown coefficient of elasticity of concrete and the non-existent condition of no initial stress, vitiate entirely formulas supported by these two props.

Here again destructive criticism would be vicious if these mathematical gymnasts were giving the best or only solution which present knowledge could produce, or if the critic did not point out a substitute. The substitute is so simple of application, in such agreement with experiments, and so logical in its derivation, that it is surprising that it has not been generally adopted. The neutral axis of reinforced concrete beams under safe loads is near the middle of the depth of the beams. If, in all cases, it be taken at the middle of the depth of the concrete beam, and if variation of intensity of stress in the concrete be taken as uniform from this neutral axis up, the formula for the resisting moment of a reinforced concrete beam becomes extremely simple and no more complex than that for a rectangular wooden beam.

The eleventh point concerns complex formulas for chimneys. It is a simple matter to find the tensile stress in that part of a plain concrete chimney between two radii on the windward side. If in this space there is inserted a rod which is capable of taking that tension at a proper unit, the safety of the chimney is assured, as far as that tensile stress is concerned. Why should frightfully complex formulas be proposed, which bring in the unknowable modulus of elasticity of concrete and can only be solved by stages or dependence on the calculations of some one else?

The twelfth point concerns deflection calculations. As is well known, deflection does not play much of a part in the design of beams. Sometimes, however, the passing requirement of a certain floor construction is the amount of deflection under a given load. Professor Gaetano Lanza has given some data on recorded deflections of reinforced concrete beams.* He has also worked out the theoretical deflections on various assumptions. An attempt to reconcile the observed deflections with one of several methods of calculating stresses led him to the conclusion that:

"The observations made thus far are not sufficient to furnish the means for determining the actual distribution of the stresses, and

* "Stresses in Reinforced Concrete Beams," *Journal*, Am. Soc. Mech. Engrs., Mid-October, 1909.

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hence for the deduction of reliable formulæ for the computation of the direct stresses, shearing stresses, diagonal stresses, deflections, position of the neutral axis, etc., under a given load."

Professor Lanza might have gone further and said that the observations made thus far are sufficient to show the hopelessness of deriving a formula that will predict accurately the deflection of a reinforced concrete beam. The wide variation shown by two beam tests cited by him, in which the beams were identical, is, in itself, proof of this.

Taking the data of these tests, and working out the modulus of elasticity from the recorded deflections, as though the beams were of plain concrete, values are found for this modulus which are not out of agreement with the value of that variable modulus as determined by other means. Therefore, if the beams be considered as plain concrete beams, and an average value be assumed for the modulus or coefficient of elasticity, a deflection may be found by a simple calculation which is an average of that which may be expected. Here again, simple theory is better than complex, because of the ease with which it may be applied, and because it gives results which are just as reliable.

The thirteenth point concerns the elastic theory as applied to a reinforced concrete arch. This theory treats a reinforced concrete arch as a spring. In order to justify its use, the arch or spring is considered as having fixed ends. The results obtained by the intricate methods of the elastic theory and the simple method of the equilibrium polygon, are too nearly identical to justify the former when the arch is taken as hinged at the ends.

The assumption of fixed ends in an arch is a most extravagant one, because it means that the abutments must be rigid, that is, capable of taking bending moments. Rigidity in an abutment is only effected by a large increase in bulk, whereas strength in an arch ring is greatly augmented by the addition of a few inches to its thickness. By the elastic theory, the arch ring does not appear to need as much strength as by the other method, but additional stability is needed in the abutments in order to take the bending moments. This latter feature is not dwelt on by the elastic theorists.

In the ordinary arch, the criterion by which the size of abutment is gauged, is the location of the line of pressure. It is difficult and

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expensive to obtain depth enough in the base of the abutment to keep this line within the middle third, when only the thrust of the arch is considered. If, in addition to the thrust, there is a bending moment which, for many conditions of loading, further displaces the line of pressure toward the critical edge, the difficulty and expense are increased. It cannot be gainsaid that a few cubic yards of concrete added to the ring of an arch will go much further toward strengthening the arch than the same amount of concrete added to the two abutments.

In reinforced concrete there are ample grounds for the contention that the carrying out of a nice theory, based on nice assumptions and the exact determination of ideal stresses, is of far less importance than the building of a structure which is, in every way, capable of performing its function. There are more than ample grounds for the contention that the ideal stresses worked out for a reinforced concrete structure are far from realization in this far from ideal material.

Apart from the objection that the elastic theory, instead of showing economy by cutting down the thickness of the arch ring, would show the very opposite if fully carried out, there are objections of greater weight, objections which strike at the very foundation of the theory as applied to reinforced concrete. In the elastic theory, as in the intricate beam theory commonly used, there is the assumption of an initial unstressed condition of the materials. This is not true of a beam and is still further from the truth in the case of an arch. Besides shrinkage of the concrete, which always produces unknown initial stresses, there is a still more potent cause of initial stress, namely, the settlement of the arch when the forms are removed. If the initial stresses are unknown, ideal determinations of stresses can have little meaning.

The elastic theory stands or falls according as one is able or unable to calculate accurately the deflection of a reinforced concrete beam; and it is an impossibility to calculate this deflection even approximately. The tests cited by Professor Lanza show the utter disagreement in the matter of deflections. Of those tested, two beams which were identical, showed results almost 100% apart. A theory grounded on such a shifting foundation does not deserve serious consideration. Professor Lanza's conclusions, quoted under the twelfth point, have special meaning and force when applied to a reinforced concrete arch;

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the actual distribution of the stresses cannot possibly be determined, and complex cloaks of arithmetic cannot cover this fact. The elastic theory, far from being a reliable formula, is false and misleading in the extreme.

The fourteenth point refers to temperature calculations in a reinforced concrete arch. These calculations have no meaning whatever. To give the grounds for this assertion would be to reiterate much of what has been said under the subject of the elastic arch. If the unstressed shape of an arch cannot be determined because of the unknown effect of shrinkage and settlement, it is a waste of time to work out a slightly different unstressed shape due to temperature variation, and it is a further waste of time to work out the supposed stresses resulting from deflecting that arch back to its actual shape.

If no other method of finding the approximate stresses in an arch existed, the elastic theory might be classed as the best available; but this is not the case. There is a method which is both simple and reliable. Accuracy is not claimed for it, and hence it is in accord with the more or less uncertain materials dealt with. Complete safety, however, is assured, for it treats the arch as a series of blocks, and the cementing of these blocks into one mass cannot weaken the arch. Reinforcement can be proportioned in the same manner as for chimneys, by finding the tension exerted to pull these blocks apart and then providing steel to take that tension.

The fifteenth point concerns steel in compression in reinforced concrete columns or beams. It is common practice—and it is recommended in the most pretentious works on the subject—to include in the strength of a concrete column slender longitudinal rods embedded in the concrete. To quote from one of these works:

"The compressive resistance of a hooped member exceeds the sum of the following three elements: (1) The compressive resistance of the concrete without reinforcement. (2) The compressive resistance of the longitudinal rods stressed to their elastic limit. (3) The compressive resistance which would have been produced by the imaginary longitudinals at the elastic limit of the hooping metal, the volume of the imaginary longitudinals being taken as 2.4 times that of the hooping metal."

This does not stand the test, either of theory or practice; in fact, it is far from being true. Its departure from the truth is great

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enough and of serious enough moment to explain some of the worst accidents in the history of reinforced concrete.

It is a nice theoretical conception that the steel and the concrete act together to take the compression, and that each is accommodating enough to take just as much of the load as will stress it to just the right unit. Here again, initial stress plays an important part. The shrinkage of the concrete tends to put the rods in compression, the load adds more compression on the slender rods and they buckle, because of the lack of any adequate stiffening, long before the theorists' ultimate load is reached.

There is no theoretical or practical consideration which would bring in the strength of the hoops after the strength of the concrete between them has been counted. All the compression of a column must, of necessity, go through the disk of concrete between the two hoops (and the longitudinal steel). No additional strength in the hoops can affect the strength of this disk, with a given spacing of the hoops. It is true that shorter disks will have more strength, but this is a matter of the spacing of the hoops and not of their sectional area, as the above quotation would make it appear.

Besides being false theoretically, this method of investing phantom columns with real strength is wofully lacking in practical foundation. Even the assumption of reinforcing value to the longitudinal steel rods is not at all borne out in tests. Designers add enormously to the calculated strength of concrete columns when they insert some longitudinal rods. It appears to be the rule that real columns are weakened by the very means which these designers invest with reinforcing properties. Whether or not it is the rule, the mere fact that many tests have shown these so-called reinforced concrete columns to be weaker than similar plain concrete columns is amply sufficient to condemn the practice of assuming strength which may not exist. Of all parts of a building, the columns are the most vital. The failure of one column will, in all probability, carry with it many others stronger than itself, whereas a weak and failing slab or beam does not put an extra load and shock on the neighboring parts of a structure.

In Bulletin No. 10 of the University of Illinois Experiment Station,* a plain concrete column, 9 by 9 in. by 12 ft., stood an ultimate crushing load of 2 004 lb. per sq. in. Column 2, identical in

* Page 14, column 8.

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size, and having four ⅝-in. rods embedded in the concrete, stood 1 557 lb. per sq. in. So much for longitudinal rods without hoops. This is not an isolated case, but appears to be the rule; and yet, in reading the literature on the subject, one would be led to believe that longitudinal steel rods in a plain concrete column add greatly to the strength of the column.

A paper, by Mr. M. O. Withey, before the American Society for Testing Materials, in 1909, gave the results of some tests on concrete-steel and plain concrete columns. (The term, concrete-steel, is used because this particular combination is not "reinforced" concrete.) One group of columns, namely, $W1$ to $W3$, 10½ in. in diameter, 102 in. long, and circular in shape, stood an average ultimate load of 2 600 lb. per sq. in. These columns were of plain concrete. Another group, namely, $E1$ to $E3$, were octagonal in shape, with a short diameter (12 in.), their length being 120 in. These columns contained nine longitudinal rods, ⅝ in. in diameter, and ¼-in. steel rings every foot. They stood an ultimate load averaging 2 438 lb. per sq. in. This is less than the column with no steel and with practically the same ratio of slenderness.

In some tests on columns made by the Department of Buildings, of Minneapolis, Minn.*, Test A was a 9 by 9-in. column, 9 ft. 6 in. long, with ten longitudinal, round rods, ½ in. in diameter, and 1½-in. by $\frac{3}{16}$-in. circular bands (having two ⅛-in. rivets in the splice), spaced 4 in. apart, the circles being 7 in. in diameter. It carried an ultimate load of 130 000 lb., which is much less than half "the compressive resistance of a hooped member," worked out according to the authoritative quotation before given. Another similar column stood a little more than half that "compressive resistance." Five of the seventeen tests on the concrete-steel columns, made at Minneapolis, stood less than the plain concrete columns. So much for the longitudinal rods, and for hoops which are not close enough to stiffen the rods; and yet, in reading the literature on the subject, any one would be led to believe that longitudinal rods and hoops add enormously to the strength of a concrete column.

The sixteenth indictment against common practice is in reference to flat slabs supported on four sides. Grashof's formula for flat plates has no application to reinforced concrete slabs, because it is derived

* *Engineering News*, December 3d, 1908.

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for a material strong in all directions and equally stressed. The strength of concrete in tension is almost nil, at least, it should be so considered. Poisson's ratio, so prominent in Grashof's formula, has no meaning whatever in steel reinforcement for a slab, because each rod must take tension only; and instead of a material equally stressed in all directions, there are generally sets of independent rods in only two directions. In a solution of the problem given by a high English authority, the slab is assumed to have a bending moment of equal intensity along its diagonal. It is quite absurd to assume an intensity of bending clear into the corner of a slab, and on the very support equal to that at its center. A method published by the writer some years ago has not been challenged. By this method strips are taken across the slab and the moment in them is found, considering the limitations of the several strips in deflection imposed by those running at right angles therewith. This method shows (as tests demonstrate) that when the slab is oblong, reinforcement in the long direction rapidly diminishes in usefulness. When the ratio is 1:1½, reinforcement in the long direction is needless, since that in the short direction is required to take its full amount. In this way French and other regulations give false results, and fail to work out.

If the writer is wrong in any or all of the foregoing points, it should be easy to disprove his assertions. It would be better to do this than to ridicule or ignore them, and it would even be better than to issue reports, signed by authorities, which commend the practices herein condemned.

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DISCUSSION

Mr. Wright. JOSEPH WRIGHT, M. AM. SOC. C. E. (by letter).—If, as is expected, Mr. Godfrey's paper serves to attract attention to the glaring inconsistencies commonly practiced in reinforced concrete designs, and particularly to the careless detailing of such structures, he will have accomplished a valuable purpose, and will deserve the gratitude of the Profession.

No engineer would expect a steel bridge to stand up if the detailing were left to the judgment or convenience of the mechanics of the shop, yet in many reinforced concrete designs but little more thought is given to the connections and continuity of the steel than if it were an unimportant element of the structure. Such examples, as illustrated by the retaining wall in Fig. 2, are common, the reinforcing bars of the counterfort being simply hooked by a 4-in. **U**-bend around those of the floor and wall slabs, and penetrating the latter only from 8 to 12 in. The writer can cite an example which is still worse—that of a **T**-wall, 16 ft. high, in which the vertical reinforcement of the wall slab consisted of $\frac{3}{4}$-in. bars, spaced 6 in. apart. The wall slab was 8 in. thick at the top and only 10 in. at the bottom, yet the $\frac{3}{4}$-in. vertical bars penetrated the floor slab only 8 in., and were simply hooked around its lower horizontal bars by 4-in. **U**-bends. Amazing as it may appear, this structure was designed by an engineer who is well versed in the theories of reinforced concrete design. These are only two examples from a long list which might be cited to illustrate the carelessness often exhibited by engineers in detailing reinforced concrete structures.

In reinforced concrete work the detailer has often felt the need of some simple and efficient means of attaching one bar to another, but, in its absence, it is inexcusable that he should resort to such makeshifts as are commonly used. A simple **U**-hook on the end of a bar will develop only a small part of the strength of the bar, and, of course, should not be relied on where the depth of penetration is inadequate; and, because of the necessity of efficient anchorage of the reinforcing bars where one member of a structure unites with another, it is believed that in some instances economy might be subserved by the use of shop shapes and shop connections in steel, instead of the ordinary reinforcing bars. Such cases are comparatively few, however, for the material in common use is readily adapted to the design, in the ordinary engineering structure, and only requires that its limitations be observed, and that the designer be as conscientious and consistent in detailing as though he were designing in steel.

This paper deserves attention, and it is hoped that each point therein will receive full and free discussion, but its main purport is a plea for simplicity, consistency, and conservatism in design, with which the writer is heartily in accord.

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Mr. Russell.

S. Bent Russell, M. Am. Soc. C. E. (by letter).—The author has given expression in a forcible way to feelings possessed no doubt by many careful designers in the field in question. The paper will serve a useful purpose in making somewhat clearer the limitations of reinforced concrete, and may tend to bring about a more economical use of reinforcing material.

It is safe to say that in steel bridges, as they were designed in the beginning, weakness was to be found in the connections and details, rather than in the principal members. In the modern advanced practice of bridge design the details will be found to have some excess of strength over the principal members. It is probable that the design of reinforced concrete structures will take the same general course, and that progress will be made toward safety in minor details and economy in principal bars.

Many of the author's points appear to be well taken, especially the first, the third, and the eighth.

In regard to shear bars, if it is assumed that vertical or inclined bars add materially to the strength of short deep beams, it can only be explained by viewing the beam as a framed structure or truss in which the compression members are of concrete and the tension members of steel. It is evident that, as generally built, the truss will be found to be weak in the connections, more particularly, in some cases, in the connections between the tension and compression members, as mentioned in the author's first point.

It appears to the writer that this fault may be aggravated in the case of beams with top reinforcement for compression; this is scarcely touched on by the author. In such a case the top and bottom chords are of steel, with a weakly connected web system which, in practice, is usually composed of stirrup rods looped around the principal bars and held in position by the concrete which they are supposed to strengthen.

While on this phase of the subject, it may be proper to call attention to the fact that the Progress Report of the Special Committee on Concrete and Reinforced Concrete* may well be criticised for its scant attention to the case of beams reinforced on the compression side. No limitations are specified for the guidance of the designer, but approval is given to loading the steel with its full share of top-chord stress.†

In certain systems of reinforcement now in use, such as the Kahn and Cummings systems, the need for connections between the web system and the chord member is met to some degree, as is generally known. On the other hand, however, these systems do not provide for such intensity of pressure on the concrete at the points of connection as must occur by the author's demonstration in his first point. The author's criticisms on some other points would also apply to such

* *Transactions*, Am. Soc. C. E., Vol. LXVI, p. 431.

† *Loc. cit.*, p. 448.

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Mr. Russell. systems, and it is not necessary to state that one weak detail will limit the strength of the truss.

The author has only condemnation for the use of longitudinal rods in concrete columns (Point 15). It would seem that if the longitudinal bars are to carry a part of the load they must be supported laterally by the concrete, and, as before, in the beam, it may be likened to a framed structure in which the web system is formed of concrete alone, or of a framework of poorly connected members, and the concrete and steel must give mutual support in a way not easy to analyze. It is scarcely surprising that the strength of such a structure is sometimes less than that shown by concrete alone.

In the Minneapolis tests, quoted by the author, there are certain points which should be noted, in fairness to columns reinforced longitudinally. Only four columns thus reinforced failed below the strength shown by concrete alone, and these were from 52 to 63 days old only, while the plain concrete was 98 days old. There was nothing to hold the rods in place in these four columns except the concrete and the circular hoops surrounding them. On the other hand, all the columns in which the hooping was hooked around the individual rods showed materially greater strength than the plain concrete, although perhaps one should be excepted, as it was 158 days old and showed a strength of only 2 250 lb. per sq. in., or 12% more than the plain concrete.*

In considering a column reinforced with longitudinal rods and hoops, it is proper to remark that the concrete not confined by the steel ought not to be counted as aiding the latter in any way, and that, consequently, the bond of the outside bars is greatly weakened.

In view of these considerations, it may be found economical to give the steel reinforcement of columns some stiffness of its own by sufficiently connected lateral bracing. The writer would suggest, further, that in beams where rods are used in compression a system of web members sufficiently connected should be provided, so that the strength of the combined structure would be determinate.

To sum up briefly, columns and short deep beams, especially when the latter are doubly reinforced, should be designed as framed structures, and web members should be provided with stronger connections than have been customary.

Mr. Worcester. J. R. Worcester, M. Am. Soc. C. E. (by letter).—This paper is of value in calling attention to many of the bad practices to be found in reinforced concrete work, and also in that it gives an opportunity for discussing certain features of design, about which engineers do not agree. A free discussion of these features will tend to unify methods. Several of the author's indictments, however, hit at practices which were discarded long ago by most designers, and are not recommended

* *Engineering News*, Dec. 3d, 1908.

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by any good authorities; the implication that they are in general use is unwarranted. Mr. Worcester.

The first criticism, that of bending rods at a sharp angle, may be said to be of this nature. Drawings may be made without indicating the curve, but in practice metal is seldom bent to a sharp angle. It is undoubtedly true that in every instance a gradual curve is preferable.

The author's second point, that a suitable anchorage is not provided for bent-up rods at the ends of a beam, may also be said to be a practice which is not recommended or used in the best designs.

The third point, in reference to the counterforts of retaining walls, is certainly aimed at a very reprehensible practice which should not be countenanced by any engineer.

The fourth, fifth, and sixth items bring out the fact that undoubtedly there has been some confusion in the minds of designers and authors on the subject of shear in the steel. The author is wholly justified in criticising the use of the shearing stress in the steel ever being brought into play in reinforced concrete. Referring to the report of the Special Committee on Concrete and Reinforced Concrete, on this point, it seems as if it might have made the intention of the Committee somewhat clearer had the word, tensile, been inserted in connection with the stress in the shear reinforcing rods. In considering a beam of reinforced concrete in which the shearing stresses are really diagonal, there is compression in one case and tension in another; and, assuming that the metal must be inserted to resist the tensile portion of this stress, it is not essential that it should necessarily be wholly parallel to the tensile stress. Vertical tensile members can prevent the cracking of the beam by diagonal tension, just as in a Howe truss all the tensile stresses due to shear are taken in a vertical direction, while the compressive stresses are carried in the diagonal direction by the wooden struts. The author seems to overlook the fact, however, that the reinforced concrete beam differs from the Howe truss in that the concrete forms a multiple system of diagonal compression members. It is not necessary that a stirrup at one point should carry all the vertical tension, as this vertical tension is distributed by the concrete. There is no doubt about the necessity of providing a suitable anchorage for the vertical stirrups, and such is definitely required in the recommendations of the Special Committee.

The cracks which the author refers to as being necessary before the reinforcing material is brought into action, are just as likely to occur in the case of the bent-up rods with anchors at the end, advocated by him. While his method may be a safe one, there is also no question that a suitable arrangement of vertical reinforcement may be all that is necessary to make substantial construction.

With reference to the seventh point, namely, methods of calculating moments, it might be said that it is not generally considered good

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Mr. Worcester.

practice to reduce the positive moments at the center of a span to the amount allowable in a beam fully fixed at the end, and if provision is made for a negative moment over supports sufficient to develop the stresses involved in complete continuity, there is usually a considerable margin of safety, from the fact of the lack of possible fixedness of the beams at the supports. The criticism is evidently aimed at practice not to be recommended.

As to the eighth point, the necessary width of a beam in order to transfer, by horizontal shear, the stress delivered to the concrete from the rods, it might be well worth while for the author to take into consideration the fact that while the bonding stress is developed to its full extent near the ends of the beam, it very frequently happens that only a portion of the total number of rods are left at the bottom, the others having been bent upward. It may be that the width of a beam would not be sufficient to carry the maximum bonding stress on the total number of rods near its center, and yet it may have ample shearing strength on the horizontal planes. The customary method of determining the width of the beams so that the maximum horizontal shearing stress will not be excessive, seems to be a more rational method than that suggested by Mr. Godfrey.

Referring to the tenth and fourteenth points, it would be interesting to know whether the author proportions his steel to take the remaining tension without regard to the elongation possible at the point where it is located, considering the neutral axis of the section under the combined stress. Take, for instance, a chimney: If the section is first considered to be homogeneous material which will carry tension and compression equally well, and the neutral axis is found under the combined stresses, the extreme tensile fiber stress on the concrete will generally be a matter of 100 or 200 lb. Evidently, if steel is inserted to replace the concrete in tension, the corresponding stress in the steel cannot be more than from 1 500 to 3 000 lb. per sq. in. If sufficient steel is provided to keep the unit stress down to the proper figure, there can be little criticism of the method, but if it is worked to, say, 16 000 lb. per sq. in., it is evident that the result will be a different position for the neutral axis, invalidating the calculation and resulting in a greater stress in compression on the concrete.

Mr. Mensch.

L. J. Mensch, M. Am. Soc. C. E. (by letter).—Much of the poor practice in reinforced concrete design to which Mr. Godfrey calls attention is due, in the writer's opinion, to inexperience on the part of the designer.

It is true, however, that men of high standing, who derided reinforced concrete only a few years ago, now pose as reinforced concrete experts, and probably the author has the mistakes of these men in mind.

The questions which he propounds were settled long ago by a great many tests, made in various countries, by reliable authorities, although

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Mr. Mensch.

the theoretical side is not as easily answered; but it must be borne in mind that the stresses involved are mostly secondary, and, even in steel construction, these are difficult of solution. The stresses in the web of a deep steel girder are not known, and the web is strengthened by a liberal number of stiffening angles, which no expert can figure out to a nicety. The ultimate strength of built-up steel columns is not known, frequently not even within 30%; still less is known of the strength of columns consisting of thin steel casings, or of the types used in the Quebec Bridge. It seems to be impossible to solve the problem theoretically for the simplest case, but had the designer of that bridge known of the tests made by Hodgkinson more than 40 years ago, that accident probably would not have happened.

Practice is always ahead of theory, and the writer claims that, with the great number of thoroughly reliable tests made in the last 20 years, the man who is really informed on this subject will not see any reason for questioning the points brought out by Mr. Godfrey.

The author is right in condemning sharp bends in reinforcing rods. Experienced men would not think of using them, if only for the reason that such sharp bends are very expensive, and that there is great likelihood of breaking the rods, or at least weakening them. Such sharp bends invite cracks.

Neither is there any question in regard to the advantage of continuing the bent-up rods over the supports. The author is manifestly wrong in stating that the reinforcing rods can only receive their increments of stress when the concrete is in tension. Generally, the contrary happens. In the ordinary adhesion test, the block of concrete is held by the jaws of the machine and the rod is pulled out; the concrete is clearly in compression.

The underside of continuous beams is in compression near the supports, yet no one will say that steel rods cannot take any stress there. It is quite surprising to learn that there are engineers who still doubt the advisability of using bent-up bars in reinforced concrete beams. Disregarding the very thorough tests made during the last 18 years in Europe, attention is called to the valuable tests on thirty beams made by J. J. Harding, M. Am. Soc. C. E., for the Chicago, Milwaukee and St. Paul Railroad.* All the beams were reinforced with about ¾% of steel. Those with only straight rods, whether they were plain or patented bars, gave an average shearing strength of 150 lb. per sq. in. Those which had one-third of the bars bent up gave an average shearing strength of 200 lb. per sq. in., and those which had nearly one-half of the rods bent up gave an average shearing strength of 225 lb. per sq. in. Where the bent bars were continued over the supports, higher ultimate values were obtained than where some of the rods were stopped off near the supports; but in every case bent-up bars showed a greater carrying capacity than straight rods.

* *Journal* of the Western Society of Engineers, 1906.

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Mr. Mensch.

The writer knows also of a number of tests with rods fastened to anchor-plates at the end, but the tests showed that they had only a slight increase of strength over straight rods, and certainly made a poorer showing than bent-up bars. The use of such threaded bars would increase materially the cost of construction, as well as the time of erection.

The writer confesses that he never saw or heard of such poor practices as mentioned in the author's third point. On the other hand, the proposed design of counterforts in retaining walls would not only be very expensive and difficult to install, but would also be a decided step backward in mechanics. This proposition recalls the trusses used before the introduction of the Fink truss, in which the load from the upper chord was transmitted by separate members directly to the abutments, the inventor probably going on the principle that the shortest way is the best. There are in the United States many hundreds of rectangular water tanks. Are these held by any such devices? And as they are not thus held, and inasmuch as there is no doubt that they must carry the stress when filled with water, it is clear that, as long as the rods from the sides are strong enough to carry the tension and are bent with a liberal radius into the front wall and extended far enough to form a good anchorage, the connection will not be broken. The same applies to retaining walls. It would take up too much time to prove that the counterfort acts really as a beam, although the forces acting on it are not as easily found as those in a common beam.

The writer does not quite understand the author's reference to shear rods. Possibly he means the longitudinal reinforcement, which it seems is sometimes calculated to carry 10 000 lb. per sq. in. in shear. The writer never heard of such a practice.

In regard to stirrups, Mr. Godfrey seems to be in doubt. They certainly do not act as the rivets of a plate girder, nor as the vertical rods of a Howe truss. They are best compared with the dowel pins and bolts of a compound wooden beam. The writer has seen tests made on compound concrete beams separated by copper plates and connected only by stirrups, and the strength of the combination was nearly the same as that of beams made in one piece.

Stirrups do not add much to the strength of the beams where bent bars are used, but the majority of tests show a great increase of strength where only straight reinforcing bars are used. Stirrups are safeguards against poor concrete and poor workmanship, and form a good connection where concreting is interrupted through inclemency of weather or other causes. They absolutely prevent shrinkage cracks between the stem and the flange of T-beams, and the separation of the stem and slab in case of serious fires. For the latter reason, the writer condemns the use of simple U-bars, and arranges all his stirrups so that they extend from 6 to 12 in. into the slabs. Engineers

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are warned not to follow the author's advice with regard to the omission of stirrups, but to use plenty of them in their designs, or sooner or later they will thoroughly repent it. Mr Mensch.

In regard to bending moments in continuous beams, the writer wishes to call attention to the fact that at least 99% of all reinforced structures are calculated with a reduction of 25% of the bending moment in the center, which requires only 20% of the ordinary bending moment of a freely supported beam at the supports. There may be some engineers who calculate a reduction of 33%; there are still some ultra-confident men, of little experience, who compute a reduction of 50%; but, inasmuch as most designers calculate with a reduction of only 25%, too great a factor of safety does not result, nor have any failures been observed on that account.

In the case of slabs which are uniformly loaded by earth or water pressure, the bending moments are regularly taken as $\frac{w\,l^2}{24}$ in the center and $\frac{w\,l^2}{12}$ at the supports. The writer never observed any failure of continuous beams over the supports, although he has often noticed failures in the supporting columns directly under the beams, where these columns are light in comparison with the beams. Failure of slabs over the supports is common, and therefore the writer always places extra rods over the supports near the top surface.

The width of the beams which Mr. Godfrey derives from his simple rule, that is, the width equals the sum of the peripheries of the reinforcing rods, is not upheld by theory or practice. In the first place, this width would depend on the kind of rods used. If a beam is reinforced by three $\frac{7}{8}$-in. round bars, the width, according to his formula, would be 8.2 in. If the beam is reinforced by six $\frac{5}{8}$-in. bars which have the same sectional area as the three $\frac{7}{8}$-in. bars, then the width should be 12 in., which is ridiculous and does not correspond with tests, which would show rather a better behavior for the six bars than for the three larger bars in a beam of the same width.

It is surprising to learn that there are engineers who still advocate such a width of the stem of T-beams that the favorable influence of the slab may be dispensed with, although there were many who did this 10 or 12 years ago.

It certainly can be laid down as an axiom that the man who uses complicated formulas has never had much opportunity to design or build in reinforced concrete, as the design alone might be more expensive than the difference in cost between concrete and structural steel work.

The author attacks the application of the elastic theory to reinforced concrete arches. He evidently has not made very many designs in which he used the elastic theory, or he would have found that the

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Mr. Mensch.

abutments need be only from three to four times thicker than the crown of the arch (and, therefore, their moments of inertia from 27 to 64 times greater), when the deformation of the abutments becomes negligible in the elastic equations. Certainly, the elastic theory gives a better guess in regard to the location of the line of pressure than any guess made without its use. The elastic theory was fully proved for arches by the remarkable tests, made in 1897 by the Austrian Society of Engineers and Architects, on full-sized arches of 70-ft. span, and the observed deflections and lateral deformations agreed exactly with the figured deformation.

Tests on full-sized arches also showed that the deformations caused by temperature changes agree with the elastic theory, but are not as great for the whole mass of the arch as is commonly assumed. The elastic theory enables one to calculate arches much more quickly than any graphical or guess method yet proposed.

Hooped columns are a patented construction which no one has the right to use without license or instructions from M. Considère, who clearly states that his formulas are correct only for rich concrete and for proper percentages of helical and longitudinal reinforcement, which latter must have a small spacing, in order to prevent the deformation of the core between the hoops. With these limitations his formulas are correct.

Mr. Godfrey brings up some erratic column tests, and seems to have no confidence in reinforced concrete columns. The majority of column tests, however, show an increase of strength by longitudinal reinforcement. In good concrete the longitudinal reinforcement may not be very effective or very economical, but it safeguards the strength in poorly made concrete, and is absolutely necessary on account of the bending stresses set up in such columns, due to the monolithic character of reinforced concrete work.

Mr. Godfrey does not seem to be familiar with the tests made by good authorities on square slabs of reinforced concrete and of cast iron, which latter material is also deficient in tensile strength. These tests prove quite conclusively that the maximum bending moment per linear foot may be calculated by the formulas, $\frac{w\,l^2}{32}$ or $\frac{w\,l^2}{20}$, according to the degree of fixture of the slabs at the four sides. Inasmuch as fixed ends are rarely obtained in practice, the formula, $\frac{w\,l^2}{24}$, is generally adopted, and the writer cannot see any reason to confuse the subject by the introduction of a new method of calculation.

Mr. Clifford.

WALTER W. CLIFFORD, JUN. AM. SOC. C. E. (by letter).—Some of Mr. Godfrey's criticisms of reinforced concrete practice do not seem to be well taken, and the writer begs to call attention to a few points which seem to be weak. In Fig. 1, the author objects to the use of

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diagonal bars for the reason that, if the diagonal reinforcement is stressed to the allowable limit, these bars bring the bearing on the concrete, at the point where the diagonal joins the longitudinal reinforcement, above a safe value. The concrete at the point of juncture must give, to some extent, and this would distribute the bearing over a considerable length of rod. In some forms of patented reinforcement an additional safeguard is furnished by making the diagonals of flat straps. The stress in the rods at this point, moreover, is not generally the maximum allowable stress, for considerable is taken out of the rod by adhesion between the point of maximum stress and that of juncture. Mr. Clifford.

Mr. Godfrey wishes to remedy this by replacing the diagonals by rods curved to a radius of from twenty to thirty times their diameter. In common cases this radius will be about equal to the depth of the beam. Let this be assumed to be true. It cannot be assumed that these rods take any appreciable vertical shear until their slope is 30° from the horizontal, for before this the tension in the rod would be more than twice the shear which causes it. Therefore, these curved rods, assuming them to be of sufficient size to take, as a vertical component, the shear on any vertical plane between the point where it slopes 30° and its point of maximum slope, would need to be spaced at, approximately, one-half the depth of the beam. Straight rods of equivalent strength, at 45° with the axis of the beam, at this same spacing (which would be ample), would be 10% less in length.

Mr. Godfrey states:

"Of course a reinforcing rod in a concrete beam receives its stress by increments imparted by the grip of the concrete; but these increments can only be imparted where the tendency of the concrete is to stretch."

He then overlooks the fact that at the end of a beam, such as he has shown, the maximum tension is diagonal, and at the neutral axis, not at the bottom; and the rod is in the best position to resist failure on the plane, *AB*, if its end is sufficiently well anchored. That this rod should be anchored is, as he states, undoubtedly so, but his implied objection to a bent end, as opposed to a nut, seems to the writer to be unfounded. In some recent tests, on rods bent at right angles, at a point 5 diameters distant from the end, and with a concrete backing, stress was developed equal to the bond stress on a straight rod embedded for a length of about 30 diameters, and approximately equal to the elastic limit of the rod, which, for reinforcing purposes, is its ultimate stress.

Concerning the vertical stirrups to which Mr. Godfrey refers, there is no doubt that they strengthen beams against failure by diagonal tension or, as more commonly known, shear failures. That they are not effective in the beam as built is plain, for, if one con-

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Mr. Clifford. siders a vertical plane between the stirrups, the concrete must resist the shear on this plane, unless dependence is placed on that in the longitudinal reinforcement. This, the author states, is often done, but the practice is unknown to the writer, who does not consider it of any value; certainly the stirrups cannot aid.

Suppose, however, that the diagonal tension is above the ultimate stress for the concrete, failure of the concrete will then occur on planes perpendicular to the line of maximum tension, approximately 45° at the end of the beam. If the stirrups are spaced close enough, however, and are of sufficient strength so that these planes of failure all cut enough steel to take as tension the vertical shear on the plane, then these cracks will be very minute and will be distributed, as is the case in the center of the lower part of the beam. These stirrups will then take as tension the vertical shear on any plane, and hold the beam together, so that the friction on these planes will keep up the strength of the concrete in horizontal shear. The concrete at the end of a simple beam is better able to take horizontal shear than vertical, because the compression on a horizontal plane is greater than that on a vertical plane. This idea concerning the action of stirrups falls under the ban of Mr. Godfrey's statement, that any member which "cannot act until failure has started, is not a proper element of design," but this is not necessarily true. For example, Mr. Godfrey says "the steel in the tension side of the beam should be considered as taking all the tension." This is undoubtedly true, but it cannot take place until the concrete has failed in tension at this point. If used, vertical tension members should be considered as taking all the vertical shear, and, as Mr. Godfrey states, they should certainly have their ends anchored so as to develop the strength for which they have been calculated.

The writer considers diagonal reinforcement to be the best for shear, and it should be used, especially in all cases of "unit" reinforcement; but, in some cases, stirrups can and do answer in the manner suggested; and, for reasons of practical construction, are sometimes best with "loose rod" reinforcement.

Mr. Meem. J. C. Meem, M. Am. Soc. C. E. (by letter).—The writer believes that there are some very interesting points in the author's somewhat iconoclastic paper which are worthy of careful study, and, if it be shown that he is right in most of, or even in any of, his assumptions, a further expression of approval is due to him. Few engineers have the time to show fully, by a process of *reductio ad absurdum,* that all the author's points are, or are not, well considered or well founded, but the writer desires to say that he has read this paper carefully, and believes that its fundamental principles are well grounded. Further, he believes that intricate mathematical formulas have no place in practice. This is particularly true where these elaborate mathematical calculations

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are founded on assumptions which are never found in practice or experiment, and which, even in theory, are extremely doubtful, and certainly are not possible within those limits of safety wherein the engineer is compelled to work. Mr. Meem.

The writer disagrees with the author in one essential point, however, and that is in the wholesale indictment of special reinforcement, such as stirrups, shear rods, etc. In the ordinary way in which these rods are used, they have no practical value, and their theoretical value is found only when the structure is stressed beyond its safe limits; nevertheless, occasions may arise when they have a definite practical value, if properly designed and placed, and, therefore, they should not be discriminated against absolutely.

Quoting the author, that "destructive criticism is of no value unless it offers something in its place," and in connection with the author's tenth point, the writer offers the following formula which he has always used in conjunction with the design of reinforced concrete slabs and beams. It is based on the formula for rectangular wooden beams, and assumes that the beam is designed on the principle that concrete in tension is as strong as that in compression, with the understanding that sufficient steel shall be placed on the tension side to make this true, thus fixing the neutral axis, as the author suggests, in the middle of the depth, that is, $M = \frac{1}{8}\, b\, d^2\, S$, M, of course, being the bending moment, and b and d, the breadth and depth, in inches. S is usually taken at from 400 to 600 lb., according to the conditions. In order to obtain the steel necessary to give the proper tensile strength to correspond with the compression side, the compression and tension areas of the beam are equated, that is

$$\tfrac{1}{12}\, b\, d^2\, S = a \times \left(\frac{d}{2} - x_{11}\right) \times S_{11},$$

where

a = the area of steel per linear foot,
x_{11} = the distance from the center of the steel to the outer fiber, and
S_{11} = the strength of the steel in tension.

Then for a beam, 12 in. wide,

$$d^2\, S = a\, S_{11} \left(\frac{d}{2} - x_{11}\right),$$

or

$$a = \frac{d^2\, S}{S_{11}\left(\frac{d}{2} - x_{11}\right)}.$$

Carrying this to its conclusion, we have, for example, in a beam 12 in. deep and 12 in. wide,

$S = 500$,
$S_{11} = 15\,000$,
$x_{11} = 2\tfrac{1}{2}$ in.
$a = 1.37$ sq. in. per ft.

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Mr. Meem.

The writer has used this formula very extensively, in calculating new work and also in checking other designs built or to be built, and he believes its results are absolutely safe. There is the further fact to its credit, that its simplicity bars very largely the possibility of error from its use. He sees no reason to introduce further complications into such a formula, when actual tests will show results varying more widely than is shown by a comparison between this simple formula and many more complicated ones.

Mr. Myers.

George H. Myers, Jun. Am. Soc. C. E. (by letter).—This paper brings out a number of interesting points, but that which strikes the writer most forcibly is the tenth, in regard to elaborate theories and complicated formulas for beams and slabs. The author's stand for simplicity in this regard is well taken. A formula for the design of beams and slabs need not be long or complicated in any respect. It can easily be obtained from the well-known fact that the moment at any point divided by the distance between the center of compression and the center of tension at that point gives the tension (or compression) in the beam.

The writer would place the neutral axis from 0.42 to 0.45 of the effective depth of the beam from the compression side rather than at the center, as Mr. Godfrey suggests. This higher position of the neutral axis is the one more generally shown by tests of beams. It gives the formula $M = 0.86\ d\ As\ f$, or $M = 0.85\ d\ As\ f$, which the writer believes is more accurate than $M = \frac{5}{6}\ d\ As\ f$, or $0.83\frac{1}{3}\ d\ As\ f$, which would result if the neutral axis were taken at the center of the beam.

d = depth of the beam from the compression side to the center of the steel;

As = the area of the steel;

and f = the allowable stress per square inch in the steel.

The difference, however, is very slight, the results from the two formulas being proportional to the two factors, $83\frac{1}{3}$ and 85 or 86. This formula gives the area of steel required for the moment. The percentage of steel to be used can easily be obtained from the allowable stresses in the concrete and the steel, and the dimensions of the beam can be obtained in the simplest manner. This formula is used with great success by one of the largest firms manufacturing reinforcing materials and designing concrete structures. It is well-known to the Profession, and the reason for using any other method, involving the Greek alphabet and many assumptions, is unknown to the writer. The only thing to assume—if it can be called assuming when there are so many tests to locate it—is the position of the neutral axis. A slight difference in this assumption affects the resulting design very little, and is inappreciable, from a practical point of view. It can be

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safely said that the neutral axis is at, or a little above, the center of the beam. Mr. Myers.

Further, it would seem that the criticism to the effect that the initial stress in the concrete is neglected is devoid of weight. As far as the designer is concerned, the initial stress is allowed for. The values for the stresses used in design are obtained from tests on blocks of concrete which have gone through the process of setting. Whatever initial stress exists in concrete due to this process of setting exists also in these blocks when they are tested. The value of the breaking load on concrete given by any outside measuring device used in these tests, is the value of that stress over and above this initial stress. It is this value with which we work. It would seem that, if the initial stress is neglected in arriving at a safe working load, it would be safe to neglect it in the formula for design.

Edwin Thacher, M. Am. Soc. C. E. (by letter).—The writer will discuss this paper under the several "points" mentioned by the author. Mr. Thacher.

First Point.—At the point where the first rod is bent up, the stress in this rod runs out. The other rods are sufficient to take the horizontal stress, and the bent-up portion provides only for the vertical and diagonal shearing stresses in the concrete.

Second Point.—The remarks on the first point are also applicable to the second one. Rod 3 provides for the shear.

Third Point.—In a beam, the shear rods run through the compression parts of the concrete and have sufficient anchorage. In a counterfort, the inclined rods are sufficient to take the overturning stress. The horizontal rods support the front wall and provide for shrinkage. The vertical rods also provide for shrinkage, and assist the diagonal rods against overturning. The anchorage is sufficient in all cases, and the proposed method is no more effective.

Fourth Point.—In bridge pins, bending and bearing usually govern, but, in case a wide bar pulled on a pin between the supports close to the bar, as happens in bolsters and post-caps of combination bridges and in other locations, shear would govern. Shear rods in concrete-steel beams are proportioned to take the vertical and diagonal shearing stresses. If proportioned for less stress per square inch than is used in the bottom bars, this cannot be considered dangerous practice.

Fifth Point.—Vertical stirrups are designed to act like the vertical rods in a Howe truss. Special literature is not required on the subject; it is known that the method used gives good results, and that is sufficient.

Sixth Point.—The common method is not "to assume each shear member as taking the horizontal shear occurring in the space from

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Mr. Thacher. member to member," but to take all the shear from the center of the beam up to the bar in question.

Cracks do not necessarily endanger the safety of a beam. Any device that will prevent the cracks from opening wide enough to destroy the beam, is logical. By numerous experiments, Mr. Thaddeus Hyatt found that nuts and washers at the ends of reinforcing bars were worse than useless, and added nothing to the strength of the beams.

Seventh Point.—Beams can be designed, supported at the ends, fully continuous, or continuous to a greater or less extent, as desired. The common practice is to design slabs to take a negative moment over the supports equal to one-half the positive moment at the center, or to bend up the alternate rods. This is simple and good practice, for no beam can fail as long as a method is provided by which to take care of all the stresses without overstraining any part.

Eighth Point.—Bars in the bottom of a reinforced concrete beam are often placed too close to one another. The rule of spacing the bars not less than three diameters apart, is believed to be good practice.

Ninth Point.—To disregard the theory of T-beams, and work by rule-of-thumb, can hardly be considered good engineering.

Tenth Point.—The author appears to consider theories for reinforced concrete beams and slabs as useless refinements, but as long as theory and experiment agree so wonderfully well, theories will undoubtedly continue to be used.

Eleventh Point.—Calculations for chimneys are somewhat complex, but are better and safer than rule-of-thumb methods.

Twelfth Point.—Deflection is not very important.

Thirteenth Point.—The conclusion of the Austrian Society of Engineers and Architects, after numerous experiments, was that the elastic theory of the arch is the only true theory. No arch designed by the elastic theory was ever known to fail, unless on account of insecure foundations, therefore engineers can continue to use it with confidence and safety.

Fourteenth Point.—Calculations for temperature stresses, as per theory, are undoubtedly correct for the variations in temperature assumed. Similar calculations can also be made for shrinkage stresses, if desired. This will give a much better idea of the stresses to be provided for, than no calculations at all.

Fifteenth Point.—Experiments show that slender longitudinal rods, poorly supported, and embedded in a concrete column, add little or nothing to its strength; but stiff steel angles, securely latticed together, and embedded in the concrete column, will greatly increase its strength, and this construction is considered the most desirable when the size of the column has to be reduced to a minimum.

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Sixteenth Point.—The commonly accepted theory of slabs supported on four sides can be correctly applied to reinforced concrete slabs, as it is only a question of providing for certain moments in the slab. This theory shows that unless the slab is square, or nearly so, nothing is to be gained by such construction. Mr. Thacher.

C. A. P. TURNER, M. AM. SOC. C. E. (by letter).—Mr. Godfrey has expressed his opinion on many questions in regard to concrete construction, but he has adduced no clean-cut statement of fact or tests, in support of his views, which will give them any weight whatever with the practical matter-of-fact builder. Mr. Turner.

The usual rules of criticism place the burden of proof on the critic. Mr. Godfrey states that if his personal opinions are in error, it should be easy to prove them to be so, and seems to expect that the busy practical constructor will take sufficient interest in them to spend the time to write a treatise on the subject in order to place him right in the matter.

The writer will confine his discussion to only a few points of the many on which he disagrees with Mr. Godfrey.

First, regarding stirrups: These may be placed in the beam so as to be of little practical value. They were so placed in the majority of the tests made at the University of Illinois. Such stirrups differ widely in value from those used by Hennebique and other first-class constructors.

Mr. Godfrey's idea is that the entire pull of the main reinforcing rod should be taken up apparently at the end. When one frequently sees slabs tested, in which the steel breaks at the center, with no end anchorage whatever for the rods, the soundness of Mr. Godfrey's position may be questioned.

Again, concrete is a material which shows to the best advantage as a monolith, and, as such, the simple beam seems to be decidedly out of date to the experienced constructor.

Mr. Godfrey appears to consider that the hooping and vertical reinforcement of columns is of little value. He, however, presents for consideration nothing but his opinion of the matter, which appears to be based on an almost total lack of familiarity with such construction.

The writer will state a few facts regarding work which he has executed. Among such work have been columns in a number of buildings, with an 18-in. core, and carrying more than 500 tons; also columns in one building, which carry something like 1 100 tons on a 27-in. core. In each case there is about 1½ in. of concrete outside the core for a protective coating. The working stress on the core, if it takes the load, is approximately equal to the ultimate strength of the concrete in cubes, to say nothing of the strength of cylinders fifteen times their diameter in height. These values have been used with

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Mr. Turner. entire confidence after testing full-sized columns designed with the proper proportions of vertical steel and hooping, and are regarded by the writer as having at least double the factor of safety used in ordinary designs of structural steel.

An advantage which the designer in concrete has over his fellow-engineer in the structural steel line, lies in the fact that, with a given type of reinforcement, his members are similar in form, and when the work is executed with ordinary care, there is less doubt as to the distribution of stress through a concrete column, than there is with the ordinary structural steel column, since the core is solid and the conditions are similar in all cases.

Tests of five columns are submitted herewith. The columns varied little in size, but somewhat in the amount of hooping, with slight differences in the vertical steel. The difference between Columns 1 and 3 is nearly 50%, due principally to the increase in hooping, and to a small addition in the amount of vertical steel. As to the efficiency of hooping and vertical reinforcement, the question may be asked Mr. Godfrey, and those who share his views, whether a column without reinforcement can be cast, which will equal the strength of those, the tests of which are submitted.

Test No. 1.*

Marks on column—none.
Reinforcement—eight $1\frac{1}{8}$-in. round bars vertically.
Band spacing—9 in. vertically.
Hooped with seven 32-in. wire spirals about 2-in. raise.
Outside diameter of hoops—$14\frac{1}{2}$ in.
Total load at failure—1 360 000 lb.

Remarks.—Point of failure was about 22 in. from the top. Little indication of failure until ultimate load was reached.

Some slight breaking off of concrete near the top cap, due possibly to the cap not being well seated in the column itself.

Test No. 2.

Marks on column—Box 4.
Reinforcement—eight $1\frac{1}{8}$-in. round bars vertically.
Band spacing about 13 in. vertically.
Wire spiral about 3-in. pitch.
Point of failure about 18 in. from top.
Top of cast-iron cap cracked at four corners.
Ultimate load—1 260 000 lb.

Remarks.—Both caps apparently well seated, as was the case with all the subsequent tests.

Test No. 3.

Marks on column—4–B.
Reinforcement—eight $\frac{7}{8}$-in. round bars vertically.
Hoops—$1\frac{3}{4}$ in. x $\frac{3}{16}$ in. x 14 in. outside diameter.

* Tests made for C. A. P. Turner, by Mason D. Pratt, M. Am. Soc. C. E.

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Mr. Turner.

Band spacing—13 in. vertically.
Ultimate load—900 000 lb.
Point of failure about 2 ft. from top.
Remarks.—Concrete, at failure, considerably disintegrated, probably due to continuance of movement of machine after failure.

TEST No. 4.

Marks on column—Box 4.
Reinforcement—eight 1-in. round bars vertically.
Hoops spaced 8 in. vertically.
Wire spirals as on other columns.
Total load at failure—1 260 000 lb.
Remarks.—First indications of failure were nearest the bottom end of the column, but the total failure was, as in all other columns, within 2 ft. of the top. Large cracks in the shell of the column extended from both ends to very near the middle. This was the most satisfactory showing of all the columns, as the failure was extended over nearly the full length of the column.

TEST No. 5.

Marks on column—none.
Reinforcement—eight $\frac{7}{8}$-in. bars vertically.
Hoops spaced 10 in. vertically.
Outside diameter of hoops—14½ in.
Wire spiral as before.
Load at failure—1 100 000 lb.
Ultimate load—1 130 000 lb.
Remarks.—The main point of failure in this, as in all other columns, was within 2 ft. of the top, although this column showed some scaling off at the lower end.

In these tests it will be noted that the concrete outside of the hooped area seems to have had very little value in determining the ultimate strength; that, figuring the compression on the core area and deducting the probable value of the vertical steel, these columns exhibited from 5 000 to 7 000 lb. per sq. in. as the ultimate strength of the hooped area, not considering the vertical steel. Some of them run over 8 000 lb.

The concrete mixture was 1 part Alpena Portland cement, 1 part sand, 1½ parts buckwheat gravel and 3½ parts gravel ranging from ¼ to ¾ in. in size.

The columns were cast in the early part of December, and tested in April. The conditions under which they hardened were not particularly favorable, owing to the season of the year.

The bands used were 1¾ by ¼ in., except in the light column, where they were 1¾ by $\frac{3}{16}$ in.

In his remarks regarding the tests at Minneapolis, Minn., Mr. Godfrey has failed to note that these tests, faulty as they undoubtedly were, both in the execution of the work, and in the placing of the reinforcement, as well as in the character of the hooping used, were sufficient to satisfy the Department of Buildings that rational design took into consideration the amount of hooping and the amount of vertical

Mr. Turner.

steel, and on a basis not far from that which the writer considers reasonable practice.

Again, Mr. Godfrey seems to misunderstand the influence of Poisson's ratio in multiple-way reinforcement. If Mr. Godfrey's ideas are correct, it will be found that a slab supported on two sides, and reinforced with rods running directly from support to support, is stronger than a similar slab reinforced with similar rods crossing it diagonally in pairs. Tests of these two kinds of slabs show that those with the diagonal reinforcement develop much greater strength than those reinforced directly from support to support. Records of small test slabs of this kind will be found in the library of the Society.

Mr. Godfrey makes the good point that the accuracy of an elastic theory must be determined by the elastic deportment of the construction under load, and it seems to the writer that if authors of textbooks would pay some attention to this question and show by calculation that the elastic deportment of slabs is in keeping with their method of figuring, the gross errors in the theoretical treatment of slabs in the majority of works on reinforced concrete would be remedied.

Although he makes the excellent point noted, Mr. Godfrey very inconsistently fails to do this in connection with his theory of slabs, otherwise he would have perceived the absurdity of any method of calculating a multiple-way reinforcement by endeavoring to separate the construction into elementary beam strips. This old-fashioned method was discarded by the practical constructor many years ago, because he was forced to guarantee deflections of actual construction under severe tests. Almost every building department contains some regulation limiting the deflection of concrete floors under test, and yet no commissioner of buildings seems to know anything about calculating deflections.

In the course of his practice the writer has been required to give surety bonds of from $50 000 to $100 000 at a time, to guarantee under test both the strength and the deflection of large slabs reinforced in multiple directions, and has been able to do so with accuracy by methods which are equivalent to considering Poisson's ratio, and which are given in his book on concrete steel construction.

Until the engineer pays more attention to checking his complicated theories with facts as determined by tests of actual construction, the view, now quite general among the workers in reinforced concrete regarding him will continue to grow stronger, and their respect for him correspondingly less, until such time as he demonstrates the applicability of his theories to ordinary every-day problems.

Mr. Chapman.

Paul Chapman, Assoc. M. Am. Soc. C. E. (by letter).—Mr. Godfrey has pointed out, in a forcible manner, several bad features of textbook design of reinforced concrete beams and retaining walls. The practical engineer, however, has never used such methods of construc-

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tion. Mr. Godfrey proposes certain rules for the calculation of stresses, but there are no data of experiments, or theoretical demonstrations, to justify their use. **Mr. Chapman.**

It is also of the utmost importance to consider the elastic behavior of structures, whether of steel or concrete. To illustrate this, the writer will cite a case which recently came to his attention. A roof was supported by a horizontal 18-in. I-beam, 33 ft. long, the flanges of which were coped at both ends, and two 6 by 4-in. angles, 15 ft. long, supporting the same, were securely riveted to the web, thereby forming a frame to resist lateral wind pressure. Although the 18-in. I-beam was not loaded to its full capacity, its deflection caused an outward flexure of $\frac{3}{4}$ in. and consequent dangerous stresses in the 6 by 4-in. angle struts. The frame should have been designed as a structure fixed at the base of the struts. The importance of the elastic behavior of a structure is forcibly illustrated by comparing the contract drawings for a great cantilever bridge which spans the East River with the expert reports on the same. Due to the neglect of the elastic behavior of the structure in the contract drawings, and another cause, the average error in the stresses of 290 members was $18\frac{1}{2}\%$, with a maximum of 94 per cent.

Mr. Godfrey calls attention to the fact that stringers in railroad bridges are considered as simple beams; this is theoretically proper because the angle knees at their ends can transfer practically no flange stress. It is also to be noted that when stringers are in the plane of a tension chord, they are milled to exact lengths, and when in the plane of a compression chord, they are given a slight clearance in order to prevent arch action.

The action of shearing stresses in concrete beams may be illustrated by reference to the diagrams in Fig. 3, where the beams are loaded

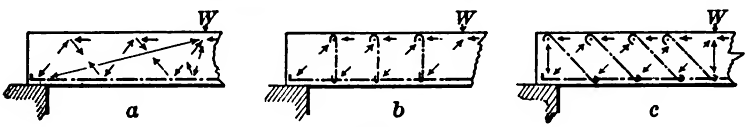

FIG. 3.

with a weight, W. The portion of W traveling to the left support, moves in diagonal lines, varying from many sets of almost vertical lines to a single diagonal. The maximum intensity of stress probably would be in planes inclined about 45°, since, considered independently, they produce the least deflection. While the load, W, remains relatively small, producing but moderate stresses in the steel in the bottom flange, the concrete will carry a considerable portion of the bottom flange tension; when the load W is largely increased, the coefficient of elasticity of the concrete in tension becomes small, or zero, if small fissures appear, and the concrete is unable to transfer the tension

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Mr. Chapman.

in diagonal planes, and failure results. For a beam loaded with a single load, W, the failure would probably be in a diagonal line near the point of application, while in a uniformly loaded beam, it would probably occur in a diagonal line near the support, where the shear is greatest.

It is evident that the introduction of vertical stirrups, as at b, or the more rational inclined stirrups, as at c, influences the action of the shearing forces as indicated, the intensity of stress at the point of connection of the stirrups being high. It is advisable to space the stirrups moderately close, in order to reduce this intensity to reasonable limits. If the assumption is made that the diagonal compression in the concrete acts in a plane inclined at 45°, then the tension in the vertical stirrups will be the vertical shear times the horizontal spacing of the stirrups divided by the distance, center to center, of the top and bottom flanges of the beam. If the stirrups are inclined at 45°, the stress in them would be 0.7 the stress in vertical stirrups with the same spacing. Bending up bottom rods sharply, in order to dispense with suspenders, is bad practice; the writer has observed diagonal cracks in the beams of a well-known building in New York City, which are due to this cause.

In several structures which the writer has recently designed, he has been able to dispense with stirrups, and, at the same time, effect a saving in concrete, by bending some of the bottom reinforcing rods and placing a bar between them and those which remain horizontal. A typical detail is shown in Fig. 4. The bend occurs at a point

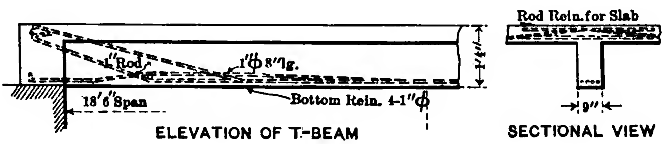

Fig. 4.

where the vertical component of the stress in the bent bars equals the vertical shear, and sufficient bearing is provided by the short cross-bar. The bars which remain horizontal throughout the beam, are deflected at the center of the beam in order to obtain the maximum effective depth. There being no shear at the center, the bars are spaced as closely as possible, and still provide sufficient room for the concrete to flow to the soffit of the beam. Two or more adjacent beams are readily made continuous by extending the bars bent up from each span, a distance along the top flanges. By this system of construction one avoids stopping a bar where the live load unit stress in adjoining bars is high, as their continual lengthening and shortening under stress would cause severe shearing stresses in the concrete surrounding the end of the short bar.

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Mr. Chapman.

The beam shown in Fig. 5 illustrates the principles stated in the foregoing, as applied to a heavier beam. The duty of the short cross-bars in this case is performed by wires wrapped around the longitudinal rods and then continued up in order to support the bars during erection. This beam, which supports a roof and partitions, etc., has

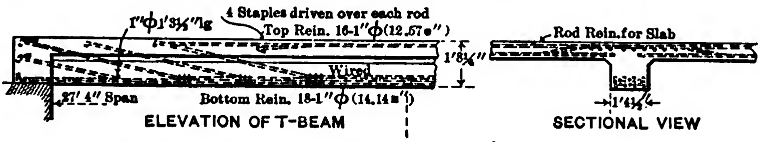

FIG. 5.

supported about 80% of the load for which it was calculated, and no hair cracks or noticeable deflection have appeared. If the method of calculation suggested by Mr. Godfrey were a correct criterion of the actual stresses, this particular beam (and many others) would have shown many cracks and noticeable deflection. The writer maintains that where the concrete is poured centinuously, or proper bond is provided, the influence of the slab as a compression flange is an actual condition, and the stresses should be calculated accordingly.

In the calculation of continuous T-beams, it is necessary to consider the fact that the moment of inertia for negative moments is small because of the lack of sufficient compressive area in the stem or web. If Mr. Godfrey will make proper provision for this point, in studying the designs of practical engineers, he will find due provision made for negative moments. It is very easy to obtain the proper amount of steel for the negative moment in a slab by bending up the bars and letting them project into adjoining spans, as shown in Figs. 4 and 5 (taken from actual construction). The practical engineer does not find, as Mr. Godfrey states, that the negative moment is double the positive moment, because he considers the live load either on one span only, or on alternate spans.

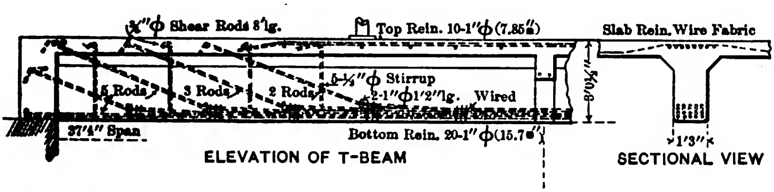

FIG. 6.

In Fig. 6 a beam is shown which has many rods in the bottom flange, a practice which Mr. Godfrey condemns. As the structure, which has about twenty similar beams, is now being built, the writer would be thankful for his criticism. Mr. Godfrey states that longitudinal steel in columns is worthless, but until definite tests are made,

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Mr. Chapman.

with the same ingredients, proportions, and age, on both plain concrete and reinforced concrete columns properly designed, the writer will accept the data of other experiments, and proportion steel in accordance with recognized formulas.

Mr. Godfrey states that the "elastic theory" is worthless for the design of reinforced concrete arches, basing his objections on the shrinkage of concrete in setting, the unreliability of deflection formulas for beams, and the lack of rigidity of the abutments. The writer, noting that concrete setting in air shrinks, whereas concrete setting in water expands, believes that if the arch be properly wetted until the setting up of the concrete has progressed sufficiently, the effect of shrinkage, on drying out, may be minimized. If the settlement of the forms themselves be guarded against during the construction of an arch, the settlement of the arch ring, on removing the forms, far from being an uncertain element, should be a check on the accuracy of the calculations and the workmanship, since the weight

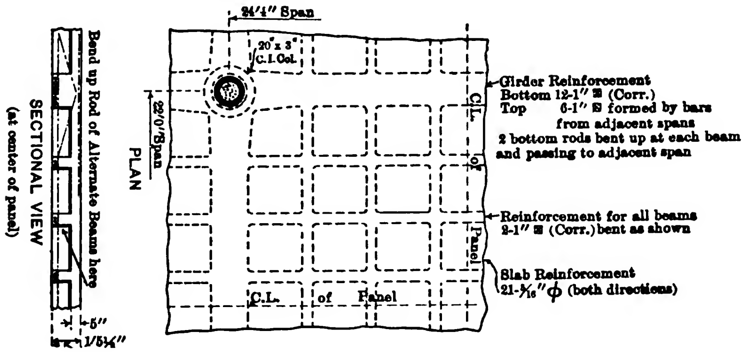

FIG. 7.

of the arch ring should produce theoretically a certain deflection. The unreliability of deflection formulas for beams is due mainly to the fact that the neutral axis of the beam does not lie in a horizontal plane throughout, and that the shearing stresses are neglected therein. While there is necessarily bending in an arch ring due to temperature, loads, etc., the extreme flanges sometimes being in tension, even in a properly designed arch, the compression exceeds the tension to such an extent that comparison to a beam does not hold true. An arch should not be used where the abutments are unstable, any more than a suspension bridge should be built where a suitable anchorage cannot be obtained.

The proper design of concrete slabs supported on four sides is a complex and interesting study. The writer has recently designed a floor construction, slabs, and beams, supported on four corners, which is simple and economical. In Fig. 7 is shown a portion of a proposed

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twelve-story building, 90 by 100 ft., having floors with a live-load capacity of 250 lb. per sq. ft. For the maximum positive bending in any panel the full load on that panel was considered, there being no live load on adjoining panels. For the maximum negative bending moment all panels were considered as loaded, and in a single line. "Checker-board" loading was considered too improbable for consideration. The flexure curves for beams at right angles to each other were similar (except in length), the tension rods in the longer beams being placed underneath those in the shorter beams. Under full load, therefore, approximately one-half of the load went to the long-span girder and the other half to the short-span girder. The girders were the same depth as the beams. For its depth the writer found this system to be the strongest and most economical of those investigated. Mr. Chapman.

E. P. Goodrich, M. Am. Soc. C. E.—The speaker heartily concurs with the author as to the large number of makeshifts constantly used by a majority of engineers and other practitioners who design and construct work in reinforced concrete. It is exceedingly difficult for the human mind to grasp new ideas without associating them with others in past experience, but this association is apt to clothe the new idea (as the author suggests) in garments which are often worse than "swaddling-bands," and often go far toward strangling proper growth. Mr. Goodrich.

While the speaker cannot concur with equal ardor with regard to all the author's points, still in many he is believed to be well grounded in his criticism. Such is the case with regard to the first point mentioned—that of the use of bends of large radius where the main tension rods are bent so as to assist in the resistance of diagonal tensile stresses.

As to the second point, provided proper anchorage is secured in the top concrete for the rod marked 3 in Fig. 1, the speaker cannot see why the concrete beneath such anchorage over the support does not act exactly like the end post of a queen-post truss. Nor can he understand the author's statement that:

"A reinforcing rod in a concrete beam receives its stress by increments imparted by the grip of the concrete; but these increments can only be imparted where the tendency of the concrete is to stretch."

The latter part of this quotation has reference to the point questioned by the speaker. In fact, the remainder of the paragraph from which this quotation is taken seems to be open to grave question, no reason being evident for not carrying out the analogy of the queen-post truss to the extreme. Along this line, it is a well-known fact that the bottom chords in queen-post trusses are useless, as far as resistance to tension is concerned. The speaker concurs, however, in the author's criticism as to the lack of anchorage usually found in most reinforcing rods, particularly those of the type mentioned in the author's second point.

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Mr. Goodrich.

This matter of end anchorage is also referred to in the third point, and is fully concurred in by the speaker, who also concurs in the criticism of the arrangement of the reinforcing rods in the counterforts found in many retaining walls. The statement that "there is absolutely no analogy between this triangle [the counterfort] and a beam" is very strong language, and it seems risky, even for the best engineer, to make such a statement as does the author when he characterizes his own design (Diagram *b* of Fig. 2) as "the only rational and the only efficient design possible." Several assumptions can be made on which to base the arrangement of reinforcement in the counterfort of a retaining wall, each of which can be worked out with equal logic and with results which will prevent failure, as has been amply demonstrated by actual experience.

The speaker heartily concurs in the author's fourth point, with regard to the impossibility of developing anything like actual shear in the steel reinforcing rods of a concrete beam; but he demurs when the author affirms, as to the possibility of so-called shear bars being stressed in "shear or tension," that "either would be absurd and impossible without greatly overstressing some other part."

As to the fifth point, reference can be given to more than one place in concrete literature where explanations of the action of vertical stirrups may be found, all of which must have been overlooked by the author. However, the speaker heartily concurs with the author's criticism as to the lack of proper connection which almost invariably exists between vertical "web" members and the top and bottom chords of the imaginary Howe truss, which holds the nearest analogy to the conditions existing in a reinforced concrete beam with vertical "web" reinforcement.

The author's reasoning as to the sixth point must be considered as almost wholly facetious. He seems to be unaware of the fact that concrete is relatively very strong in pure shear. Large numbers of tests seem to demonstrate that, where it is possible to arrange the reinforcing members so as to carry largely all tensile stresses developed through shearing action, at points where such tensile stresses cannot be carried by the concrete, reinforced concrete beams can be designed of ample strength and be quite within the logical processes developed by the author, as the speaker interprets them.

The author's characterization of the results secured at the University of Illinois Experiment Station, and described in its Bulletin No. 29, is somewhat misleading. It is true that the wording of the original reference states in two places that "stirrups do not come into action, at least not to any great extent, until a diagonal crack has formed," but, in connection with this statement, the following quotations must be read:

"The tests were planned with a view of determining the amount of stress (tension and bond) developed in the stirrups. However, for

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various reasons, the results are of less value than was expected. The beams were not all made according to the plans. In the 1907 tests, the stirrups in a few of the beams were poorly placed and even left exposed at the face of the beam, and a variation in the temperature conditions of the laboratory also affected the results. It is evident from the results that the stresses developed in the stirrups are less than they were calculated to be, and hence the layout was not well planned to settle the points at issue. The tests, however, give considerable information on the effectiveness of stirrups in providing web resistance." Mr. Goodrich.

"A feature of the tests of beams with stirrups is slow failure, the load holding well up to the maximum under increased deflection and giving warning of its condition."

"Not enough information was obtained to determine the actual final occasion of failure in these tests. In a number of cases the stirrups slipped, in others it seemed that the steel in the stirrups was stretched beyond its elastic limit, and in some cases the stirrups broke."

"As already stated, slip of stirrups and insufficient bond resistance were in many cases the immediate cause of diagonal tension failures, and therefore bond resistance of stirrups may be considered a critical stress."

These quotations seem to indicate much more effectiveness in the action of vertical stirrups than the author would lead one to infer from his criticisms. It is rather surprising that he advocates so strongly the use of a suspension system of reinforcement. That variety has been used abroad for many years, and numerous German experiments have proved with practical conclusiveness that the suspension system is not as efficient as the one in which vertical stirrups are used with a proper arrangement. An example is the conclusion arrived at by Mörsch, in "Eisenbetonbau," from a series of tests carried out by him near the end of 1906:

"It follows that with uniform loads, the suspended system of reinforcement does not give any increase of safety against the appearance of diagonal tension cracks, or the final failure produced by them, as compared with straight rods without stirrups, and that stirrups are so much the more necessary."

Again, with regard to tests made with two concentrated loads, he writes:

"The stirrups, supplied on one end, through their tensile strength, hindered the formation of diagonal cracks and showed themselves essential and indispensable elements in the * * * [suspension] system. The limit of their effect is, however, not disclosed by these experiments. * * * In any case, from the results of the second group of experiments can be deduced the facts that the bending of the reinforcement according to the theory concerning the diagonal tensile stress * * * is much more effective than according to the suspension theory, in this case the ultimate loads being in the proportion of 34: 23.4: 25.6."

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Mr. Goodrich.

It is the speaker's opinion that the majority of the failures described in Bulletin No. 29 of the University of Illinois Experiment Station, which are ascribed to diagonal tension, were actually due to deficient anchorage of the upper ends of the stirrups.

Some years ago the speaker demonstrated to his own satisfaction, the practical value of vertical stirrups. Several beams were built identical in every respect except in the size of wire used for web reinforcement. The latter varied from nothing to ⅜-in. round by five steps. The beams were similarly tested to destruction, and the ultimate load and type of failure varied in a very definite ratio to the area of vertical steel.

With regard to the author's seventh point, the speaker concurs heartily as far as it has to do with a criticism of the usual design of continuous beams, but his experience with beams designed as suggested by the author is that failure will take place eventually by vertical cracks starting from the top of the beams close to the supports and working downward so as to endanger very seriously the strength of the structures involved. This type of failure was prophesied by the speaker a number of years ago, and almost every examination which he has lately made of concrete buildings, erected for five years or longer and designed practically in accord with the author's suggestion, have disclosed such dangerous features, traceable directly to the ideas described in the paper. These ideas are held by many other engineers, as well as being advocated by the author. The only conditions under which the speaker would permit of the design of a continuous series of beams as simple members would be when they are entirely separated from each other over the supports, as by the introduction of artificial joints produced by a double thickness of sheet metal or building paper. Even under these conditions, the speaker's experience with separately moulded members, manufactured in a shop and subsequently erected, has shown that similar top cracking may take place under certain circumstances, due to the vertical pressures caused by the reactions at the supports. It is very doubtful whether the action described by the author, as to the type of failure which would probably take place with his method of design, would be as described by him, but the beams would be likely to crack as described above, in accordance with the speaker's experience, so that the whole load supported by the beam would be carried by the reinforcing rods which extend from the beam into the supports and are almost invariably entirely horizontal at such points. The load would thus be carried more nearly by the shearing strength of the steel than is otherwise possible to develop that type of stress. In every instance the latter is a dangerous element.

This effect of vertical abutment action on a reinforced beam was very marked in the beam built of bricks and tested by the speaker, as described in the discussion* of the paper by John S. Sewell, M. Am.

* *Transactions*, Am. Soc. C. E., Vol. LVI, p. 343.

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Soc. S. E., on "The Economical Design of Reinforced Concrete Floor Systems for Fire-Resisting Structures." That experiment also went far toward showing the efficacy of vertical stirrups. Mr. Goodrich.

The same discussion also contains a description of a pair of beams tested for comparative purposes, in one of which adhesion between the concrete and the main reinforcing rods was possible only on the upper half of the exterior surfaces of the latter rods except for short distances near the ends. Stirrups were used, however. The fact that the beam, which was theoretically very deficient in adhesion, failed in compression, while the similar beam without stirrups, but with the most perfect adhesion, and anchorage obtainable through the use of large end hooks, failed in bond, has led the speaker to believe that, in affording adhesive resistance, the upper half of a bar is much more effective than the lower half. This seems to be demonstrated further by comparisons between simple adhesion experiments and those obtained with beams.

The speaker heartily concurs with the author's criticism of the amount of time usually given by designing engineers to the determination of the adhesive stresses developed in concrete beams, but, according to the speaker's recollection, these matters are not so poorly treated in some books as might be inferred by the author's language. For example, both Bulletin No. 29, of the University of Illinois, and Mörsch, in "Eisenbetonbau," give them considerable attention.

The ninth point raised by the author is well taken. Too great emphasis cannot be laid on the inadequacy of design disclosed by an examination of many T-beams.

Such ready concurrence, however, is not lent to the author's tenth point. While it is true that, under all usual assumptions, except those made by the author, an extremely simple formula for the resisting moment of a reinforced concrete beam cannot be obtained, still his formula falls so far short of fitting even with approximate correctness the large number of well-known experiments which have been published, that a little more mathematical gymnastic ability on the part of the author and of other advocates of extreme simplicity would seem very necessary, and will produce structures which are far more economical and amply safe structurally, compared with those which would be produced in accordance with his recommendations.

As to the eleventh point, in regard to the complex nature of the formulas for chimneys and other structures of a more or less complex beam nature, the graphical methods developed by numerous German and Italian writers are recommended, as they are fully as simple as the rather crude method advocated by the author, and are in almost identical accord with the most exacting analytical methods.

With regard to the author's twelfth point, concerning deflection calculations, it would seem that they play such a small part in reinforced concrete design, and are required so rarely, that any engineer

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Mr. Goodrich.

who finds it necessary to make analytical investigations of possible deflections would better use the most precise analysis at his command, rather than fall back on simpler but much more approximate devices such as the one advocated by the author.

Much of the criticism contained in the author's thirteenth point, concerning the application of the elastic theory to the design of concrete arches, is justified, because designing engineers do not carry the theory to its logical conclusion nor take into account the actual stresses which may be expected from slight changes of span, settlements of abutments, and unexpected amounts of shrinkage in the arch ring or ribs. Where conditions indicate that such changes are likely to take place, as is almost invariably the case unless the foundations are upon good rock and the arch ring has been concreted in relatively short sections, with ample time and device to allow for initial shrinkage; or unless the design is arranged and the structure erected so that hinges are provided at the abutments to act during the striking of the falsework, which hinges are afterward wedged or grouted so as to produce fixation of the arch ends—unless all these points are carefully considered in the design and erection, it is the speaker's opinion that the elastic theory is rarely properly applicable, and the use of the equilibrium polygon recommended by the author is much preferable and actually more accurate. But there must be consistency in its use, as well, that is, consistency between methods of design and erection.

The author's fourteenth point—the determination of temperature stresses in a reinforced concrete arch—is to be considered in the same light as that described under the foregoing points, but it seems a little amusing that the author should finally advocate a design of concrete arch which actually has no hinges, namely, one consisting of practically rigid blocks, after he has condemned so heartily the use of the elastic theory.

A careful analysis of the data already available with regard to the heat conductivity of concrete, applied to reinforced concrete structures like arches, dams, retaining walls, etc., in accordance with the well-known but somewhat intricate mathematical formulas covering the laws of heat conductivity and radiation so clearly enunciated by Fourier, has convinced the speaker that it is well within the bounds of engineering practice to predict and care for the stresses which will be produced in structures of the simplest forms, at least as far as they are affected by temperature changes.

The speaker concurs with the author in his criticism, contained in the fifteenth point, with regard to the design of the steel reinforcement in columns and other compression members. While there may be some question as to the falsity or truth of the theory underlying certain types of design, it is unquestioned that some schemes of arrangement undoubtedly produce designs with dangerous properties. The speaker

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has several times called attention to this point, in papers and discussions, and invariably in his own practice requires that the spacing of spirals, hoops, or ties be many times less than that usually required by building regulations and found in almost every concrete structure. Mörsch, in his "Eisenbetonbau," calls attention to the fact that very definite limits should be placed on the maximum size of longitudinal rods as well as on their minimum diameters, and on the maximum spacing of ties, where columns are reinforced largely by longitudinal members. He goes so far as to state that: Mr. Goodrich.

"It is seen from * * * [the results obtained] that an increase in the area of longitudinal reinforcement does not produce an increase in the breaking strength to the extent which would be indicated by the formula. * * * In inexperienced hands this formula may give rise to constructions which are not sufficiently safe."

Again, with regard to the spacing of spirals and the combination with them of longitudinal rods, in connection with some tests carried out by Mörsch, the conclusion is as follows:

"On the whole, the tests seem to prove that when the spirals are increased in strength, their pitch must be decreased, and the cross-section or number of the longitudinal rods must be increased."

In the majority of cases, the spiral or band spacing is altogether too large, and, from conversations with Considère, the speaker understands that to be the inventor's view as well.

The speaker makes use of the scheme mentioned by the author in regard to the design of flat slabs supported on more than two sides (noted in the sixteenth point), namely, that of dividing the area into strips, the moments of which are determined so as to produce computed deflections which are equal in the two strips running at right angles at each point of intersection. This method, however, requires a large amount of analytical work for any special case, and the speaker is mildly surprised that the author cannot recommend some simpler method so as to carry out his general scheme of extreme simplification of methods and design.

If use is to be made at all of deflection observations, theories, and formulas, account should certainly be taken of the actual settlements and other deflections which invariably occur in Nature at points of support. These changes of level, or slope, or both, actually alter very considerably the stresses as usually computed, and, in all rigorous design work, should be considered.

On the whole, the speaker believes that the author has put himself in the class with most iconoclasts, in that he has overshot his mark. There seems to be a very important point, however, on which he has touched, namely, the lack of care exercised by most designers with regard to those items which most nearly correspond with the so-called "details" of structural steel work, and are fully as important in rein-

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Mr. Goodrich.

forced concrete as in steel. It is comparatively a small matter to proportion a simple reinforced concrete beam at its intersection to resist a given moment, but the carrying out of that item of the work is only a start on the long road which should lead through the consideration of every detail, not the least important of which are such items as most of the sixteen points raised by the author.

The author has done the profession a great service by raising these questions, and, while full concurrence is not had with him in all points, still the speaker desires to express his hearty thanks for starting what is hoped will be a complete discussion of the really vital matter of detailing reinforced concrete design work.

Mr. Beyer.

ALBIN H. BEYER, ESQ.—Mr. Goodrich has brought out very clearly the efficiency of vertical stirrups. As Mr. Godfrey states that explanations of how stirrups act are conspicuous in the literature of reinforced concrete by their absence, the speaker will try to explain their action in a reinforced concrete beam.

It is well known that the internal static conditions in reinforced concrete beams change to some extent with the intensity of the direct or normal stresses in the steel and concrete. In order to bring out his point, the speaker will trace, in such a beam, the changes in the internal static conditions due to increasing vertical loads.

Let Fig. 8 represent a beam reinforced by horizontal steel rods of

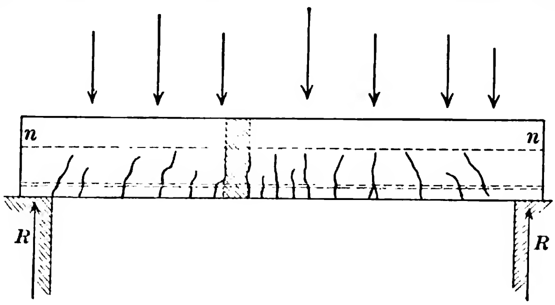

FIG. 8.

such diameter that there is no possibility of failure from lack of adhesion of the concrete to the steel. The beam is subjected to the vertical loads, ΣP. For low unit stresses in the concrete, the neutral surface, $n\ n$, is approximately in the middle of the beam. Gradually increase the loads, ΣP, until the steel reaches an elongation of from 0.01 to 0.02 of 1%, corresponding to tensile stresses in the steel of from 3 000 to 6 000 lb. per sq. in. At this stage plain concrete would have reached its ultimate elongation. It is known, however, that reinforced concrete, when well made, can sustain without rupture much greater elongations; tests have shown that its ultimate elongation may be as high as 0.1 of 1%, corresponding to tensions in steel of 30 000 lb. per sq. in.

Reinforced concrete structures ordinarily show tensile cracks at very much lower unit stresses in the steel. The main cause of these cracks is as follows: Reinforced concrete setting in dry air undergoes considerable shrinkage during the first few days, when it has very little resistance. This tendency to shrink being opposed by the reinforcement at a time when the concrete does not possess the necessary strength or ductility, causes invisible cracks or planes of weakness in the concrete. These cracks open and become visible at very low unit stresses in the steel. Mr. Beyer.

Increase the vertical loads, ΣP, and the neutral surface will rise and small tensile cracks will appear in the concrete below the neutral surface (Fig. 8). These cracks are most numerous in the central part of the span, where they are nearly vertical. They decrease in number at the ends of the span, where they curve slightly away from the perpendicular toward the center of the span. The formation of these tensile cracks in the concrete relieves it at once of its highly stressed condition.

It is impossible to predict the unit tension in the steel at which these cracks begin to form. They can be detected, though not often visible, when the unit tensions in the steel are as low as from 10 000 to 16 000 lb. per sq. in. As soon as the tensile cracks form, though invisible, the neutral surface approaches the position in the beam assigned to it by the common theory of flexure, with the tension in the concrete neglected. The internal static conditions in the beam are now modified to the extent that the concrete below the neutral surface is no longer continuous. The common theory of flexure can no longer be used to calculate the web stresses.

To analyze the internal static conditions developed, the speaker will treat as a free body the shaded portion of the beam shown in Fig. 8, which lies between two tensile cracks.

In Fig. 9 are shown all the forces which act on this free body, $C\ b\ b'\ C'$.

At any section, let

C or C' represent the total concrete compression;

T or T' represent the total steel tension;

J or J' represent the total vertical shear;

P represent the total vertical load for the length, b-b';

and let $\Delta\ T = T' - T = C' - C$ represent the total transverse shear for the length, b-b'.

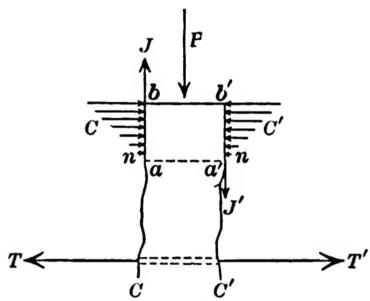

FIG. 9.

Assuming that the tension cracks extend to the neutral suface, $n\ n$, that portion of the beam $C\ b\ b'\ C'$, acts as a cantilever fixed at

Mr. Beyer.

ab and *a′ b′*, and subjected to the unbalanced steel tension, ΔT. The vertical shear, *J*, is carried mainly by the concrete above the neutral surface, very little of it being carried by the steel reinforcement. In the case of plain webs, the tension cracks are the forerunners of the sudden so-called diagonal tension failures produced by the snapping off, below the neutral surface, of the concrete cantilevers. The logical method of reinforcing these cantilevers is by inserting vertical steel in the tension side. The vertical reinforcement, to be efficient, must be well anchored, both in the top and in the bottom of the beam. Experience has solved the problem of doing this by the use of vertical steel in the form of stirrups, that is, U-shaped rods. The horizontal reinforcement rests in the bottom of the U.

Sufficient attention has not been paid to the proper anchorage of the upper ends of the stirrups. They should extend well into the compression area of the beam, where they should be properly anchored. They should not be too near the surface of the beam. They must not be too far apart, and they must be of sufficient cross-section to develop the necessary tensile forces at not excessive unit stresses. A working tension in the stirrups which is too high, will produce a local disintegration of the cantilevers, and give the beam the appearance of failure due to diagonal tension. Their distribution should follow closely that of the vertical or horizontal shear in the beam. Practice must rely on experiment for data as to the size and distribution of stirrups for maximum efficiency.

The maximum shearing stress in a concrete beam is commonly computed by the equation:

$$v = \frac{V}{\frac{7}{8}\, b\, d} \quad \ldots\ldots\ldots\ldots (1)$$

Where *d* is the distance from the center of the reinforcing bars to the surface of the beam in compression:

b = the width of the flange, and

V = the total vertical shear at the section.

This equation gives very erratic results, because it is based on a continuous web. For a non-continuous web, it should be modified to

$$v = \frac{V}{K\, b\, d} \quad \ldots\ldots\ldots\ldots (2)$$

In this equation $K\, b\, d$ represents the concrete area in compression. The value of K is approximately equal to 0.4.

Three large concrete beams with web reinforcement, tested at the University of Illinois,* developed an average maximum shearing resistance of 215 lb. per sq. in., computed by Equation 1. Equation 2 would give 470 lb. per sq. in.

* Bulletin No. 28, University of Illinois.

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Three T-beams, having 32 by 3½-in. flanges and 8-in. webs, tested at the University of Illinois, had maximum shearing resistances of 585, 605, and 370 lb. per. sq. in., respectively.* They did not fail in shear, although they appeared to develop maximum shearing stresses which were almost three times as high as those in the rectangular beams mentioned. The concrete and web reinforcement being identical, the discrepancy must be somewhere else. Based on a non-continuous concrete web, the shearing resistances become 385, 400, and 244 lb. per sq. in., respectively. As none of these failed in shear, the ultimate shearing resistance of concrete must be considerably higher than any of the values given. Mr. Beyer.

About thirteen years ago, Professor A. Vierendeel† developed the theory of open-web girder construction. By an open-web girder, the speaker means a girder which has a lower and upper chord connected by verticals. Several girders of this type, far exceeding solid girders in length, have been built. The theory of the open-web girder, assuming the verticals to be hinged at their lower ends, applies to the concrete beam reinforced with stirrups. Assuming that the spaces between the verticals of the girder become continually narrower, they become the tension cracks of the concrete beam..‡

JOHN C. OSTRUP, M. AM. SOC. C. E.—The author has rendered a great service to the Profession in presenting this paper. In his first point he mentions two designs of reinforced concrete beams and, inferentially, he condemns a third design to which the speaker will refer later. The designs mentioned are, first, that of a reinforced concrete beam arranged in the shape of a rod, with separate concrete blocks placed on top of it without being connected—such a beam has its strength only in the rod. It is purely a suspension, or "hog-chain" affair, and the blocks serve no purpose, but simply increase the load on the rod and its stresses. Mr. Ostrup.

The author's second design is an invention of his own, which the Profession at large is invited to adopt. This is really the same system as the first, except that the blocks are continuous and, presumably, fixed at the ends. When they are so fixed, the concrete will take compressive stresses and a certain portion of the shear, the remaining shear being transmitted to the rod from the concrete above it, but only through friction. Now, the frictional resistance between a steel rod and a concrete beam is not such as should be depended on in modern engineering designs.

The third method is that which is used by nearly all competent designers, and it seems to the speaker that, in condemning the general practice of current reinforced designs in sixteen points, the author

* Bulletin No. 12, University of Illinois, Table VI, page 27.
† Professeur de Stabilité à l'Université de Louvain.
‡ A translation of Professor Vierendeel's theory may be found in *Beton und Eisen*, Vols. X, XI, and XII, 1907.

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Mr. Ostrup.

could have saved himself some time and labor by condemning them all in one point.

What appears to be the underlying principle of reinforced concrete design is the adhesion, or bond, between the steel and the concrete, and it is that which tends to make the two materials act in unison. This is a point which has not been touched on sufficiently, and one which it was expected that Mr. Beyer would have brought out, when he illustrated certain internal static conditions. This principle, in the main, will cover the author's fifth point, wherein stirrups are mentioned, and again in the first point, wherein he asks: "Will some advocate of this type of design please state where this area can be found?"

To understand clearly how concrete acts in conjunction with steel, it is necessary to analyze the following question: When a steel rod is embedded in a solid block of concrete, and that rod is put in tension, what will be the stresses in the rod and the surrounding concrete?

The answer will be illustrated by reference to Fig. 10. It must be understood that the unit stresses should be selected so that both the concrete and the steel may be stressed in the same relative ratio. Assuming the tensile stress in the steel to be 16 000 lb. per sq. in., and the bonding value 80 lb., a simple formula will show that the length of embedment, or that part of the rod which will act, must be equal to 50 diameters of the rod.

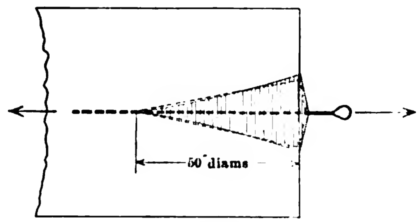

Fig. 10.

When the rod is put in tension, as indicated in Fig. 10, what will be the stresses in the surrounding concrete? The greatest stress will come on the rod at the point where it leaves the concrete, where it is a maximum, and it will decrease from that point inward until the total stress in the steel has been distributed to the surrounding concrete. At that point the rod will only be stressed back for a distance equal in length to 50 diameters, no matter how far beyond that length the rod may extend.

The distribution of the stress from the steel rod to the concrete can be represented by a cone, the base of which is at the outer face of the block, as the stresses will be zero at a point 50 diameters back, and will increase in a certain ratio out toward the face of the block, and will also, at all intermediate points, decrease radially outward from the rod.

The intensity of the maximum stress exerted on the concrete is represented by the shaded area in Fig. 10, the ordinates, measured perpendicularly to the rod, indicating the maximum resistance offered by the concrete at any point.

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If the concrete had a constant modulus of elasticity under varying stress, and if the two materials had the same modulus, the stress triangle would be bounded by straight lines (shown as dotted lines in Fig. 10); but as this is not true, the variable moduli will modify the stress triangle in a manner which will tend to make the boundary lines resemble parabolic curves. Mr. Ostrup.

A triangle thus constructed will represent by scale the intensity of the stress in the concrete, and if the ordinates indicate stresses greater than that which the concrete will stand, a portion will be destroyed, broken off, and nothing more serious will happen than that this stress triangle will adjust itself, and grip the rod farther back. This process keeps on until the end of the rod has been reached, when the triangle will assume a much greater maximum depth as it shortens; or, in other words, the disintegration of the concrete will take place here very rapidly, and the rod will be pulled out.

In the author's fourth point he belittles the use of shear rods, and states: "No hint is given as to whether these bars are in shear or in tension." As a matter of fact, they are neither in shear nor wholly in tension, they are simply in bending between the centers of the compressive resultants, as indicated in Fig. 12, and are, besides, stressed slightly in tension between these two points.

In Fig. 10 the stress triangle indicates the distribution and the intensity of the resistance in the concrete to a force acting parallel to the rod. A similar triangle may be drawn, Fig. 11, showing the resistance of the rod and the resultant distribution in the concrete to a force perpendicular to the rod. Here the original force would cause plain shear in the rod, were the latter fixed in position. Since this cannot be the case, the force will be resolved into two components, one of which will cause a tensile stress in the rod and the other will pass through the centroid of the compressive stress area. This is indicated in Fig. 11, which, otherwise, is self-explanatory.

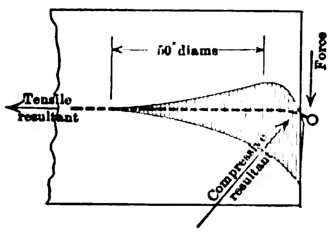

FIG. 11.

Rods are not very often placed in such a position, but where it is unavoidable, as in construction joints in the middle of slabs or beams, they serve a very good purpose; but, to obtain the best effect from them, they should be placed near the center of

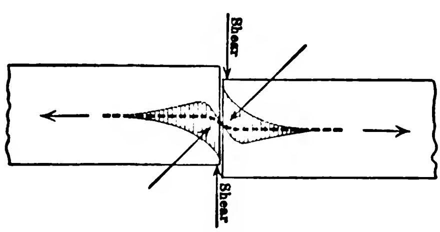

FIG. 12.

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Mr. Ostrup.

the slab, as in Fig. 12, and not near the top, as advocated by some writers.

If the concrete be overstressed at the points where the rod tends to bend, that is, if the rods are spaced too far apart, disintegration will follow; and, for this reason, they should be long enough to have more than 50 diameters gripped by the concrete.

This leads up to the author's seventh point, as to the overstressing of the concrete at the junction of the diagonal tension rods, or stirrups, and the bottom reinforcement.

Analogous with the foregoing, it is easy to lay off the stress triangles and to find the intensity of stress at the maximum points, in fact at any point, along the tension rods and the bottom chord. This is indicated in Fig. 13. These stress triangles will start on the rod 50

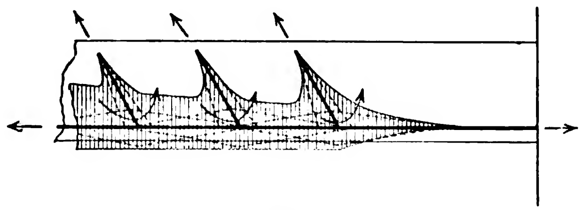

Fig. 13.

diameters back from the point in question and, although the author has indicated in Fig. 1 that only two of the three rods are stressed, there must of necessity also be some stress in the bottom rod to the left of the junction, on account of the deformation which takes place in any beam due to bending. Therefore, all three rods at the point where they are joined, are under stress, and the triangles can be laid off accordingly.

It will be noticed that the concrete will resist the compressive components, not at any specific point, but all along the various rods, and with the intensities shown by the stress triangles; also, that some of these triangles will overlap, and, hence, a certain readjustment, or superimposition, of stresses takes place.

The portion which is laid off below the bottom rods will probably not act unless there is sufficient concrete below the reinforcing bars and on the sides, and, as that is not the case in ordinary construction, it is very probable, as Mr. Goodrich has pointed out, that the concrete below the rods plays an unimportant part, and that the triangle which is now shown below the rod should be partially omitted.

The triangles in Fig. 13 show the intensity of stress in the concrete at any point, or at any section where it is wanted. They show conclusively where the components are located in the concrete, their relation to the tensile stresses in the rods, and, furthermore, that they act only in a general way at right angles to one another. This is in

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accordance with the theory of beams, that at any point in the web there are tensile and compressive stresses of equal intensity, and at right angles to one another, although in a non-homogeneous web the distribution is somewhat different. Mr. Ostrup.

After having found at the point of junction the intensity of stress, it is possible to tell whether or not a bond between the stirrups and the bottom rods is necessary, and it would not seem to be where the stirrups are vertical.

It would also seem possible to tell in what direction, if any, the bend in the inclined stirrups should be made. It is to be assumed, although not expressly stated, that the bends should curve from the center up toward the end of the beam, but an inspection of the stress triangles, Fig. 13, will show that the intensity of stress is just as great on the opposite side, and it is probable that, if any bends were required to reduce the maximum stress in the concrete, they should as likely be made on the side nearest the abutment.

From the stress triangles it may also be shown that, if the stirrups were vertical instead of inclined, the stress in the concrete on both sides would be practically equal, and that, in consequence, vertical stirrups are preferable.

The next issue raised by the author is covered in his seventh point, and relates to bending moments. He states: " * * * bending moments in so-called continuous beams are juggled to reduce them to what the designer would like to have them. This has come to be almost a matter of taste, * * * ."

The author seems to imply that such juggling is wrong. As a matter of fact, it is perfectly allowable and legitimate in every instance of beam or truss design, that is, from the standpoint of stress distribution, although this "juggling" is limited in practice by economical considerations.

In a series of beams supported at the ends, bending moments range from $\frac{w\,l^2}{8}$ at the center of each span to zero at the supports, and, in a series of cantilevers, from zero at the center of the span to $\frac{w\,l^2}{8}$ at the supports. Between these two extremes, the designer can divide, adjust, or juggle them to his heart's content, provided that in his design he makes the proper provision for the corresponding shifting of the points of contra-flexure. If that were not the case, how could ordinary bridge trusses, which have their maximum bending at the center, compare with those which, like arches, are assumed to have no bending at that point?

In his tenth point, the author proposes a method of simple designing by doing away with the complicated formulas which take account of the actual co-operation of the two materials. He states that an ideal

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Mr. Ostrup. design can be obtained in the same manner, that is, with the same formulas, as for ordinary rectangular beams; but, when he does so, he evidently fails to remember that the neutral axis is not near the center of a reinforced concrete beam under stress; in fact, with the percentage of reinforcement ordinarily used in designing—varying between three-fourths of 1% to 1½%—the neutral axis, when the beam is loaded, is shifted from 26 to 10% of the beam depth above the center. Hence, a low percentage of steel reinforcement will produce a great shifting of the neutral axis, so that a design based on the formulas advocated by the author would contain either a waste of materials, an overstress of the concrete, or an understress of the steel; in fact, an error in the design of from 10 to 26 per cent. Such errors, indeed, are not to be recommended by good engineers.

The last point which the speaker will discuss is that of the elastic arch. The theory of the elastic arch is now so well understood, and it offers such a simple and, it might be said, elegant and self-checking solution of the arch design, that it has a great many advantages, and practically none of the disadvantages of other methods.

The author's statement that the segments of an arch could be made up of loose blocks and afterward cemented together, cannot be endorsed by the speaker, for, upon such cementing together, a shifting of the lines of resistance will take place when the load is applied. The speaker does not claim that arches are maintained by the cement or mortar joining the voussoirs together, but that the lines of pressure will be materially changed, and the same calculations are not applicable to both the unloaded and the loaded arch.

It is quite true, as the author states, that a few cubic yards of concrete placed in the ring will strengthen the arch more than a like amount added to the abutments, provided, however, that this material be placed properly. No good can result from an attempt to strengthen a structure by placing the reinforcing material promiscuously. This has been tried by amateurs in bridge construction, and, in such cases, the material either increased the distance from the neutral axis to the extreme fibers, thereby reducing the original section modulus, or caused a shifting of the neutral axis followed by a large bending moment; either method weakening the members it had tried to reinforce. In other words, the mere addition of material does not always strengthen a structure, unless it is placed in the proper position, and, if so placed, it should be placed all over commensurately with the stresses, that is, the unit stresses should be reduced.

The author has criticized reinforced concrete construction on the ground that the formulas and theories concerning it are not as yet fully developed. This is quite true, for the simple reason that there are so many uncertain elements which form their basis: First, the variable quantity of the modulus of elasticity, which,

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in the concrete, varies inversely as the stress; and, second, the fact that the neutral axis in a reinforced concrete beam under changing stress is migratory. There are also many other elements of evaluation, which, though of importance, are uncertain. Mr. Ostrup.

Because the formulas are established on certain assumptions is no reason for condemning them. There are, the speaker might add, few formulas in the subject of theoretical mechanics which are not based on some assumption, and as long as the variations are such that their range is known, perfectly reliable formulas can be deduced and perfectly safe structures can be built from them.

There are a great many theorists who have recently complained about the design of reinforced concrete. It seems to the speaker that such complaints can serve no useful purpose. Reinforced concrete structures are being built in steadily increasing numbers; they are filling a long needed place; they are at present rendering great service to mankind; and they are destined to cover a field of still greater usefulness. Reinforced concrete will undoubtedly show in the future that the confidence which most engineers and others now place in it is fully merited.

Mr. Porter. HARRY F. PORTER, JUN. AM. SOC. C. E. (by letter).—Mr. Godfrey has brought forward some interesting and pertinent points, which, in the main, are well taken; but, in his zealousness, he has fallen into the error of overpersuading himself of the gravity of some of the points he would make; on the other hand, he fails to go deeply enough into others, and some fallacies he leaves untouched. Incidentally, he seems somewhat unfair to the Profession in general, in which many earnest, able men are at work on this problem, men who are not mere theorists, but have been reared in the hard school of practical experience, where refinements of theory count for little, but common sense in design counts for much—not to mention those self-sacrificing devotees to the advancement of the art, the collegiate and laboratory investigators.

Engineers will all agree with Mr. Godfrey that there is much in the average current practice that is erroneous, much in textbooks that is misleading if not fallacious, and that there are still many designers who are unable to think in terms of the new material apart from the vestures of timber and structural steel, and whose designs, therefore, are cumbersome and impractical. The writer, however, cannot agree with the author that the practice is as radically wrong as he seems to think. Nor is he entirely in accord with Mr. Godfrey in his "constructive criticism" of those practices in which he concurs, that they are erroneous.

That Mr. Godfrey can see no use in vertical stirrups or U-bars is surprising in a practical engineer. One is prompted to ask: "Can the holder of this opinion ever have gone through the experience of

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Mr. Porter. placing steel in a job, or at least have watched the operation?" If so, he must have found some use for those little members which he professes to ignore utterly.

As a matter of fact, U-bars perform the following very useful and indispensable services:

(*1*).—If properly made and placed, they serve as a saddle in which to rest the horizontal steel, thereby insuring the correct placing of the latter during the operation of concreting, not a mean function in a type of construction so essentially practical. To serve this purpose, stirrups should be made as shown in Plate III. They should be restrained in some manner from moving when the concrete strikes them. A very good way of accomplishing this is to string them on a longitudinal rod, nested in the bend at the upper end. Mr. Godfrey, in his advocacy of bowstring bars anchored with washers and nuts at the ends, fails to indicate how they shall be placed. The writer, from experience in placing steel, thinks that it would be very difficult, if not impractical, to place them in this manner; but let a saddle of U-bars be provided, and the problem is easy.

(*2*).—Stirrups serve also as a tie, to knit the stem of the beam to its flange—the superimposed slab. The latter, at best, is not too well attached to the stem by the adhesion of the concrete alone, unassisted by the steel. T-beams are used very generally, because their construction has the sanction of common sense, it being impossible to cast stem and slab so that there will be the same strength in the plane at the junction of the two as elsewhere, on account of the certainty of unevenness in settlement, due to the disproportion in their depth. There is also the likelihood that, in spite of specifications to the contrary, there will be a time interval between the pouring of the two parts, and thus a plane of weakness, where, unfortunately, the forces tending to produce sliding of the upper part of the beam on the lower (horizontal shear) are a maximum. To offset this tendency, therefore, it is necessary to have a certain amount of vertical steel, disposed so as to pass around and under the main reinforcing members and reach well up into the flange (the slab), thus getting a grip therein of no mean security. The hooking of the U-bars, as shown in Plate III, affords a very effective grip in the concrete of the slab, and this is still further enhanced by the distributing or anchoring effect of the longitudinal stringing rods. Thus these longitudinals, besides serving to hold the U-bars in position, also increase their effectiveness. They serve a still further purpose as a most convenient support for the slab bars, compelling them to take the correct position over the supports, thus automatically ensuring full and proper provision for reversed stresses. More than that, they act in compression within the middle half, and assist in tension toward the ends of the span.

Thus, by using U-bars of the type indicated, in combination with

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longitudinal bars as described, tying together thoroughly the component parts of the beam in a vertical plane, a marked increase in stiffness, if not strength, is secured. This being the case, who can gainsay the utility of the U-bar? Mr. Porter.

Of course, near the ends, in case continuity of action is realized, whereupon the stresses are reversed, the U-bars need to be inverted, although frequently inversion is not imperative with the type of U-bar described, the simple hooking of the upper ends over the upper horizontal steel being sufficient.

As to whether or not the U-bars act with the horizontal and diagonal steel to form truss systems is relatively unessential; in all probability there is some such action, which contributes somewhat to the total strength, but at most it is of minor importance. Mr. Godfrey's points as to fallacy of truss action seem to be well taken, but his conclusions in consequence—that U-bars serve no purpose—are impractical.

The number of U-bars needed is also largely a matter of practice, although subject to calculation. Practice indicates that they should be spaced no farther apart than the effective depth of the member, and spaced closer or made heavier toward the ends, in order to keep pace with cumulating shear. They need this close spacing in order to serve as an adequate saddle for the main bars, as well as to furnish, with the lighter "stringing" rods, an adequate support to the slab bars. They should have the requisite stiffness in the bends to carry their burden without appreciable sagging; it will be found that $\frac{5}{16}$ in. is about the minimum practical size, and that $\frac{1}{2}$ in. is as large as will be necessary, even for very deep beams with heavy reinforcement.

If the size and number of U-bars were to be assigned by theory, there should be enough of them to care for fully 75% of the horizontal shear, the adhesion of the concrete being assumed as adequate for the remainder.

Near the ends, of course, the inclined steel, resulting from bending up some of the horizontal bars, if it is carried well across the support to secure an adequate anchorage, or other equivalent anchorage is provided, assists in taking the horizontal shear.

The embedment, too, of large stone in the body of the beam, straddling, as it were, the neutral plane, and thus forming a lock between the flange and the stem, may be considered as assisting materially in taking horizontal shear, thus relieving the U-bars. This is a factor in the strength of actual work which theory does not take into account, and by the author, no doubt, it would be regarded as insignificant; nevertheless it is being done every day, with excellent results.

The action of these various agencies—the U-bars, diagonal steel, and embedded stone—in a concrete beam, is analogous to that of bolts or keys in the case of deepened timber beams. A concrete beam may

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Mr. Porter. be assumed, for the purposes of illustration, to be composed of a series of superimposed layers; in this case the function of the rigid material crossing these several layers normally, and being well anchored above and below, as a unifier of the member, is obvious—it acts as so many bolts joining superimposed planks forming a beam. Of course, no such lamination actually exists, although there are always incipient forces tending to produce it; these may and do manifest themselves on occasion as an actual separation in a horizontal plane at the junction of slab and stem, ordinarily the plane of greatest weakness—owing to the method of casting—as well as of maximum horizontal shear. Beams tested to destruction almost invariably develop cracks in this region. The question then naturally arises: If U-bars serve no purpose, what will counteract these horizontal cleaving forces? On the contrary, T-beams, adequately reinforced with U-bars, seem to be safeguarded in this respect; consequently, the U-bars, while perhaps adding little to the strength, as estimated by the ultimate carrying capacity, actually must be of considerable assistance, within the limit of working loads, by enhancing the stiffness and ensuring against incipient cracking along the plane of weakness, such as impact or vibratory loads might induce. Therefore, U-bars, far from being superfluous or fallacious, are, practically, if not theoretically, indispensable.

At present there seems to be considerable diversity of opinion as to the exact nature of the stress action in a reinforced concrete beam. Unquestionably, the action in the monolithic members of a concrete structure is different from that in the simple-acting, unrestrained parts of timber or structural steel construction; because in monolithic members, by the law of continuity, reverse stresses must come into play. To offset these stresses reinforcement must be provided, or cracking will ensue where they occur, to the detriment of the structure in appearance, if not in utility. Monolithic concrete construction should be tied together so well across the supports as to make cracking under working loads impossible, and, when tested to destruction, failure should occur by the gradual sagging of the member, like the sagging of an old basket. Then, and then only, can the structure be said to be adequately reinforced.

In his advocacy of placing steel to simulate a catenary curve, with end anchorage, the author is more nearly correct than in other issues he makes. Undoubtedly, an attempt should be made in every concrete structure to approximate this alignment. In slabs it may be secured simply by elevating the bars over the supports, when, if pliable enough, they will assume a natural droop which is practically ideal; or, if too stiff, they may be bent to conform approximately to this position. In slabs, too, the reinforcement may be made practically continuous, by using lengths covering several spans, and, where ends occur, by gener-

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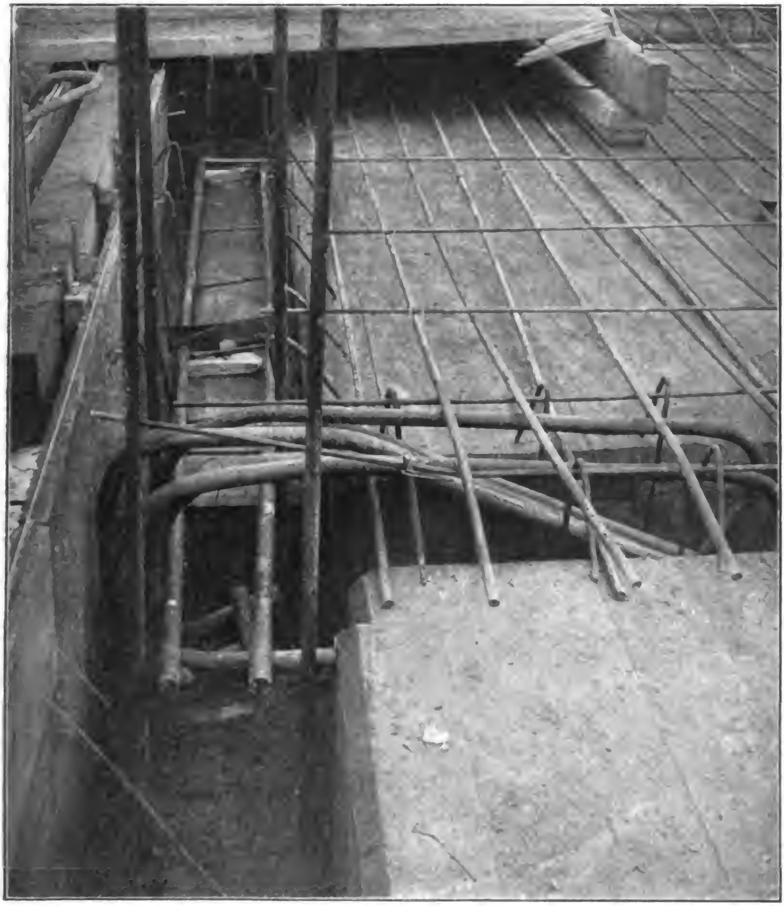

JUNCTION OF BEAM AND WALL COLUMN. REINFORCEMENT IN PLACE IN BEAM, LINTEL, AND SLAB UP TO BEAM. NOTE END ANCHORAGE OF BEAM BARS.

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Mr. Porter.

ous lapping. In beams the problem is somewhat more complicated, as it is impossible, except rarely, to bow the steel and to extend it continuously over several supports; but all or part of the horizontal steel can be bent up at about the quarter point, carried across the supports into the adjacent spans, and anchored there by bending it down at about the same angle as it is bent up on the approach, and then hooking the ends.

It is seldom necessary to adopt the scheme proposed by the author, namely, a threaded end with a bearing washer and a nut to hold the washer in place, although it is sometimes expedient, but not absolutely necessary, in end spans, where prolongation into an adjacent span is out of the question. In end spans it is ordinarily sufficient to give the bars a double reverse bend, as shown in Plate III, and possibly to clasp hooks with the horizontal steel. If steel be placed in this manner, the catenary curve will be practically approximated, the steel will be fairly developed throughout its length of embedment, and the structure will be proof against cracking. In this case, also, there is much less dependence on the integrity of the bond; in fact, if there were no bond, the structure would still develop most of its strength, although the deflection under heavy loading might be relatively greater.

The writer once had an experience which sustains this point. On peeling off the forms from a beam reinforced according to the method indicated, it was found that, because of the crowding together of the bars in the bottom, coupled with a little too stiff a mixture, the beam had hardly any concrete on the underside to grip the steel in the portion between the points of bending up, or for about the middle half of the member; consequently, it was decided to test this beam. The actual working load was first applied and no deflection, cracking, or slippage of the bars was apparent; but, as the loading was continued, deflection set in and increased rapidly for small increments of loading, a number of fine cracks opened up near the mid-section, which extended to the neutral plane, and the steel slipped just enough, when drawn taut, to destroy what bond there was originally, owing to the contact of the concrete above. At three times the live load, or 450 lb. per sq. ft., the deflection apparently reached a maximum, being about $\frac{5}{16}$ in. for a clear distance, between the supports, of 20 ft.; and, as the load was increased to 600 lb. per sq. ft., there was no appreciable increase either in deflection or cracking; whereupon, the owner being satisfied, the loading was discontinued. The load was reduced in amount to three times the working load (450 lb.) and left on over night; the next morning, there being no detectable change, the beam was declared to be sound. When the load was removed the beam recovered all but about $\frac{1}{8}$ in. of its deflection, and then repairs were made by attaching light expanded metal to the exposed bars and plastering up to form. Although nearly three years have elapsed, there have been

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Mr. Porter.

no unfavorable indications, and the owner, no doubt, has eased his mind entirely in regard to the matter. This truly remarkable showing can only be explained by the catenary action of the main steel, and some truss action by the steel which was horizontal, in conjunction with the U-bars, of which there were plenty. As before noted, the clear span was 20 ft., the width of the bay, 8 ft., and the size under the slab (which was 5 in. thick) 8 by 18 in. The reinforcement consisted of three 1⅛-in. round medium-steel bars, with ⅜-in. U-bars placed the effective depth of the member apart and closer toward the supports, the first two or three being 6 in. apart, the next two or three, 9 in., the next, 12 in., etc., up to a maximum, throughout the mid-section, of 15 in. Each U-bar was provided with a hook at its upper end, as shown in Plate III, and engaged the slab reinforcement, which in this case was expanded metal. Two of the 1⅛-in. bars were bent up and carried across the support. At the point of bending up, where they passed the single horizontal bar, which was superimposed, a lock-bar was inserted, by which the pressure of the bent-up steel against the concrete, in the region of the bend, was taken up and distributed along the horizontal bar. This feature is also shown in Fig. 14. The bars, after being carried across the support, were inclined into the adjacent span and provided with a liberal, well-rounded hook, furnishing efficient anchorage and provision for reverse stresses. This was at one end only, for—to make matters worse—the other end was a wall bearing; consequently, the benefit of continuity was denied. The bent-up bars were given a double reverse bend, as already described, carrying them around, down, in, and up, and ending finally by clasping them in the hook of the horizontal bar. This apparently stiffened up the free end, for, under the test load, its action was similar to that of the completely restrained end, thus attesting the value of this method of end-fixing.

The writer has consistently followed this method of reinforcement, with unvaryingly good results, and believes that, in some measure, it approximates the truth of the situation. Moreover, it is economical, for with the bars bent up over the supports in this manner, and positively anchored, plenty of U-bars being provided, it is possible to remove the forms with entire safety much sooner than with the ordinary methods which are not as well stirruped and only partially tied across the supports. It is also possible to put the structure into use at an earlier date. Failure, too, by the premature removal of the centers, is almost impossible with this method. These considerations more than compensate for the trouble and expense involved in connection with such reinforcement. The writer will not attempt here a theoretical analysis of the stresses incurred in the different parts of this beam, although it might be interesting and instructive.

The concrete, with the reinforcement disposed as described, may be

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Mr. Porter.

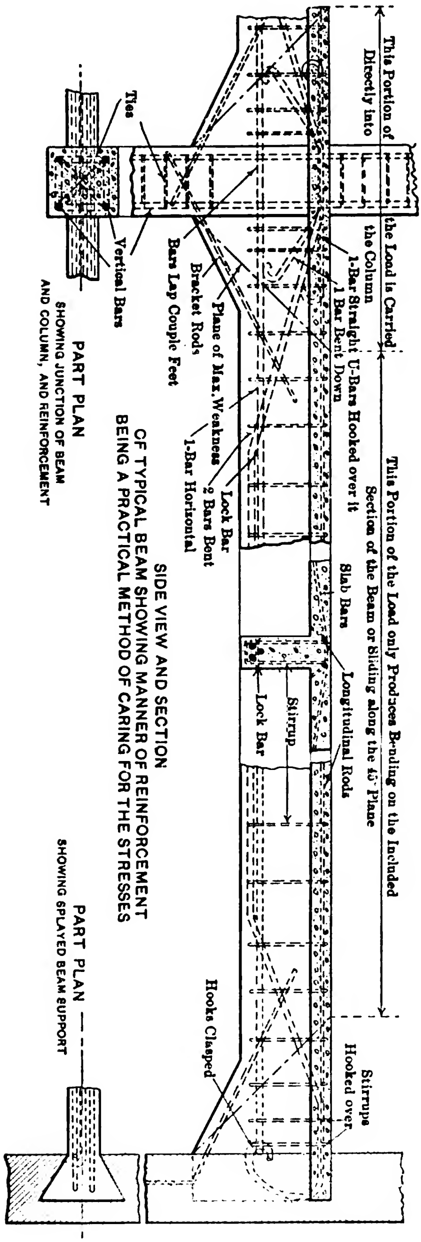

FIG. 14.

Mr. Porter. regarded as reposing on the steel as a saddle, furnishing it with a rigid jacket in which to work, and itself acting only as a stiff floor and a protecting envelope. Bond, in this case, while, of course, an adjunct, is by no means vitally important, as is generally the case with beams unrestrained in any way and in which the reinforcement is not provided with adequate end anchorage, in which case a continuous bond is apparently—at any rate, theoretically—indispensable.

An example of the opposite extreme in reinforced concrete design, where provision for reverse stresses was almost wholly lacking, is shown in the Bridgeman Brothers' Building, in Philadelphia, which collapsed while the operation of casting the roof was in progress, in the summer of 1907. The engineering world is fairly familiar with the details of this disaster, as they were noted both in the lay and technical press. In this structure, not only were U-bars almost entirely absent, but the few main bars which were bent up, were stopped short over the support. The result was that the ties between the rib and the slab, and also across the support, being lacking, some of the beams, the forms of which had been removed prematurely, cracked of their own dead weight, and, later, when the roof collapsed, owing to the deficient bracing of the centers, it carried with it each of the four floors to the basement, the beams giving way abruptly over the supports. Had an adequate tie of steel been provided across the supports, the collapse, undoubtedly, would have stopped at the fourth floor. So many faults were apparent in this structure, that, although only half of it had fallen, it was ordered to be entirely demolished and reconstructed.

The cracks in the beams, due to the action of the dead weight alone, were most interesting, and illuminative of the action which takes place in a concrete beam. They were in every case on the diagonal, at an angle of approximately 45°, and extended upward and outward from the edge of the support to the bottom side of the slab. Never was the necessity for diagonal steel, crossing this plane of weakness, more emphatically demonstrated. To the writer—an eye-witness—the following line of thought was suggested:

Should not the concrete in the region above the supports and for a distance on either side, as encompassed by the opposed 45° lines (Fig. 14), be regarded as abundantly able, of and by itself, and without reinforcing, to convey all its load into the column, leaving only the bending to be considered in the truncated portion intersected? Not even the bending should be considered, except in the case of relatively shallow members, but simply the tendency on the part of the wedge-shaped section to slip out on the 45° planes, thereby requiring sufficient reinforcement at the crossing of these planes of principal weakness to take the component of the load on this portion, tending to shove it out. This reinforcement, of course, should be anchored securely both ways; in mid-span by extending it clear through, forming a suspensory, and,

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in the other direction, by prolonging it past the supports, the concrete, in this case, along these planes, being assumed to assist partly or not at all. Mr. Porter.

This would seem to be a fair assumption. In all events, beams designed in this manner and checked by comparison with the usual methods of calculation, allowing continuity of action, are found to agree fairly well. Hence, the following statement seems to be warranted: If enough steel is provided, crossing normally or nearly so the 45° planes from the edge of the support upward and outward, to care for the component of the load on the portion included within a pair of these planes, tending to produce sliding along the same, and this steel is adequately anchored both ways, there will be enough reinforcement for every other purpose. In addition, U-bars should be provided for practical reasons.

The weak point of beams, and slabs also, fully reinforced for continuity of action, is on the under side adjacent to the edge of the support, where the concrete is in compression. Here, too, the amount of concrete available is small, having no slab to assist it, as is the case within the middle section, where the compression is in the top. Over the supports, for the width of the column, there is abundant strength, for here the steel has a leverage equal to the depth of the column; but at the very edge and for at least one-tenth of the span out, conditions are serious. The usual method of strengthening this region is to subpose brackets, suitably proportioned, to increase the available compressive area to a safe figure, as well as the leverage of the steel, at the same time diminishing the intensity of compression. Brackets, however, are frequently objectionable, and are therefore very generally omitted by careless or ignorant designers, no especial compensation being made for their absence. In Europe, especially in Germany, engineers are much more careful in this respect, brackets being nearly always included. True, if brackets are omitted, some compensation is provided by the strengthening which horizontal bars may give by extending through this region, but sufficient additional compressive resistance is rarely afforded thereby. Perhaps the best way to overcome the difficulty, without resorting to brackets, is to increase the compressive resistance of the concrete, in addition to extending horizontal steel through it. This may be done by hooping or by intermingling scraps of iron or bits of expanded metal with the concrete, thereby greatly increasing its resistance. The experiments made by the Department of Bridges of the City of New York, on the value of nails in concrete, in which results as high as 18 000 lb. per sq. in. were obtained, indicate the availability of this device; the writer has not used it, nor does he know that it has been used, but it seems to be entirely rational, and to offer possibilities.

Another practical test, which indicates the value of proper reinforce-

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Mr. Porter. ment, may be mentioned. In a storage warehouse in Canada, the floor was designed, according to the building laws of the town, for a live load of 150 lb. per sq. ft., but the restrictions being more severe than the standard American practice, limiting the lever arm of the steel to 75% of the effective depth, this was about equivalent to a 200-lb. load in the United States. The structure was to be loaded up to 400 or 500 lb. per sq. ft. steadily, but the writer felt so confident of the excess strength provided by his method of reinforcing that he was willing to guarantee the structure, designed for 150 lb., according to the Canadian laws, to be good for the actual working load. Plain, round, medium-steel bars were used. A 10-ft. panel, with a beam of 14-ft. span, and a slab 6 in. thick (not including the top coat), with ½-in. round bars, 4 in. on centers, was loaded to 900 lb. per sq. ft., at which load no measurable deflection was apparent. The writer wished to test it still further, but there was not enough cement—the material used for loading. The load, however, was left on for 48 hours, after which, no sign of deflection appearing, not even an incipient crack, it was removed. The total area of loading was 14 by 20 ft. The beam was continuous at one end only, and the slab only on one side. In other parts of the structure conditions were better, square panels being possible, with reinforcement both ways, and with continuity, both of beams and slabs, virtually in every direction, end spans being compensated by shortening. The method of reinforcing was as before indicated. The enormous strength of the structure, as proved by this test, and as further demonstrated by its use for nearly two years, can only be explained on the basis of the continuity of action developed and the great stiffness secured by liberal stirruping. Steel was provided in the middle section according to the rule, $\frac{w\,l}{8}$, the span being taken as the clear distance between the supports; two-thirds of the steel was bent up and carried across the supports, in the case of the beams, and three-fourths of the slab steel was elevated; this, with the lap, really gave, on the average, four-thirds as much steel over the supports as in the center, which, of course, was excessive, but usually an excess has to be tolerated in order to allow for adequate anchorage. Brackets were not used, but extra horizontal reinforcement, in addition to the regular horizontal steel, was laid in the bottom across the supports, which, seemingly, was satisfactory. The columns, it should be added, were calculated for a very low value, something like 350 lb. per sq. in., in order to compensate for the excess of actual live load over and above the calculated load.

This piece of work was done during the winter, with the temperature almost constantly at + 10° and dropping below zero over night. The precautions observed were to heat the sand and water, thaw out the concrete with live steam, if it froze in transporting or before it was

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FIG. 1.—SLAB AND BEAM REINFORCEMENT CONTINUOUS OVER SUPPORTS. SPAN OF BEAMS = 14 FT. SPAN OF SLABS = 12 FT. SLAB, 6 IN. THICK.

FIG. 2.—REINFORCEMENT IN PLACE OVER ONE COMPLETE FLOOR OF STORAGE WAREHOUSE. SLABS, 14 FT. SQUARE. REINFORCED TWO WAYS. NOTE CONTINUITY OF REINFORCEMENT AND ELEVATION OVER SUPPORTS. FLOOR DESIGNED FOR 150 LB. PER SQ. FT. LIVE LOAD. TESTED TO 900 LB. PER SQ. FT. WITHOUT APPRECIABLE DEFLECTION.

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settled in place, and as soon as it was placed, it was decked over and salamanders were started underneath. Thus, a job equal in every respect to warm-weather installation was obtained, it being possible to remove the forms in a fortnight. Mr. Porter.

In another part of this job (the factory annex) where, owing to the open nature of the structure, it was impossible to house it in as well as the warehouse which had bearing walls to curtain off the sides, less fortunate results were obtained. A temperature drop over night of nearly 50°, followed by a spell of alternate freezing and thawing, effected the ruin of at least the upper 2 in. of a 6-in. slab spanning 12 ft. (which was reinforced with ½-in. round bars, 4 in. on centers), and the remaining 4 in. was by no means of the best quality. It was thought that this particular bay would have to be replaced. Before deciding, however, a test was arranged, supports being provided underneath to prevent absolute failure. But as the load was piled up, to the extent of nearly 400 lb. per sq. ft., there was no sign of giving (over this span) other than an insignificant deflection of less than ¼ in., which disappeared on removing the load. This slab still performs its share of the duty, without visible defect, hence it must be safe. The question naturally arises: if 4 in. of inferior concrete could make this showing, what must have been the value of the 6 in. of good concrete in the other slabs? The reinforcing in the slab, it should be stated, was continuous over several supports, was proportioned for $\frac{w\,l}{8}$ for the clear span (about 11 ft.), and three-fourths of it was raised over the supports. This shows the value of the continuous method of reinforcing, and the enormous excess of strength in concrete structures, as proportioned by existing methods, when the reverse stresses are provided for fully and properly, though building codes may make no concession therefor.

Another point may be raised, although the author has not mentioned it, namely, the absurdity of the stresses commonly considered as occurring in tensile steel, 16 000 lb. per sq. in. for medium steel being used almost everywhere, while some zealots, using steel with a high elastic limit, are advocating stresses up to 22 000 lb. and more; even the National Association of Cement Users has adopted a report of the Committee on Reinforced Concrete, which includes a clause recommending the use of 20 000 lb. on high steel. As theory indicates, and as F. E. Turneaure, Assoc. M. Am. Soc. C. E., of the University of Wisconsin, has proven by experiment, failure of the concrete encircling the steel under tension occurs when the stress in the steel is about 5 000 lb. per sq. in. It is evident, therefore, that if a stress of even 16 000 lb. were actually developed, not to speak of 20 000 lb. or more, the concrete would be so replete with minute cracks on the tension side as to expose the embedded metal in innumerable places. Such

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Mr. Porter. cracks do not occur in work because, under ordinary working loads, the concrete is able to carry the load so well, by arch and dome action, as to require very little assistance from the steel, which, consequently, is never stressed to a point where cracking of the concrete will be induced. This being the case, why not recognize it, modify methods of design, and not go on assuming stresses which have no real existence?

The point made by Mr. Godfrey in regard to the fallacy of sharp bends is patent, and must meet with the agreement of all who pause to think of the action really occurring. This is also true of his points as to the width of the stem of T-beams, and the spacing of bars in the same. As to elastic arches, the writer is not sufficiently versed in designs of this class to express an opinion, but he agrees entirely with the author in his criticism of retaining-wall design. What the author proposes is rational, and it is hard to see how the problem could logically be analyzed otherwise. His point about chimneys, however, is not as clear.

As to columns, the writer agrees with Mr. Godfrey in many, but not in all, of his points. Certainly, the fallacy of counting on vertical steel to carry load, in addition to the concrete, has been abundantly shown. The writer believes that the sole legitimate function of vertical steel, as ordinarily used, is to reinforce the member against flexure, and that its very presence in the column, unless well tied across by loops of steel at frequent intervals, so far from increasing the direct carrying capacity, is a source of weakness. However, the case is different when a large amount of rigid vertical steel is used; then the steel may be assumed to carry all the load, at the value customary in structural steel practice, the concrete being considered only in the light of fireproofing and as affording lateral support to the steel, increasing its effective radius of gyration and thus its safe carrying capacity. In any event the load should be assumed to be carried either by the concrete or by the steel, and, if by the former, the longitudinal and transverse steel which is introduced should be regarded as auxiliary only. Vertical steel, if not counted in the strength, however, may on occasion serve a very useful practical purpose; for instance, the writer once had a job where, owing to the collection of ice and snow on a floor, which melted when the salamanders were started, the lower ends of several of the superimposed columns were eaten away, with the result that when the forms were withdrawn, these columns were found to be standing on stilts. Only four 1-in. bars were present, looped at intervals of about 1 ft., in a column 12 ft. in length and having a girth of 14 in., yet they were adequate to carry both the load of the floor above and the load incidental to construction. If no such reinforcement had been provided, however, failure would have been inevitable. Thus, again, it is shown that, where theory and experiment may fail to justify certain practices, actual experience does, and emphatically.

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Mr. Godfrey is absolutely right in his indictment of hooping as usually done, for hoops can serve no purpose until the concrete contained therein is stressed to incipient rupture; then they will begin to act, to furnish restraint which will postpone ultimate failure. Mr. Godfrey states that, in his opinion, the lamina of concrete between each hoop is not assisted; but, as a matter of fact, practically regarded, it is, the coarse particles of the aggregate bridging across from hoop to hoop; and if—as is the practice of some—considerable longitudinal steel is also used, and the hoops are very heavy, so that when the bridging action of the concrete is taken into account, there is in effect a very considerable restraining of the concrete core, and the safe carrying capacity of the column is undoubtedly increased. However, in the latter case, it would be more logical to consider that the vertical steel carried all the load, and that the concrete core, with the hoops, simply constituted its rigidity and the medium of getting the load into the same, ignoring, in this event, the direct resistance of the concrete. Mr. Porter.

What seems to the writer to be the most logical method of reinforcing concrete columns remains to be developed; it follows along the lines of supplying tensile resistance to the mass here and there throughout, thus creating a condition of homogeneity of strength. It is precisely the method indicated by the experiments already noted, made by the Department of Bridges of the City of New York, whereby the compressive resistance of concrete was enormously increased by intermingling wire nails with it. Of course, it is manifestly out of the question, practically and economically, to reinforce column concrete in this manner, but no doubt a practical and an economical method will be developed which will serve the same purpose. The writer knows of one promient reinforced concrete engineer, of acknowledged judgment, who has applied for a patent in which expanded metal is used to effect this very purpose; how well this method will succeed remains to be seen. At any rate, reinforcement of this description seems to be entirely rational, which is more than can be said for most of the current standard types.

Mr. Godfrey's sixteenth point, as to the action in square panels, seems also to the writer to be well taken; he recollects analyzing Mr. Godfrey's narrow-strip method at the time it appeared in print, and found it rational, and he has since had the pleasure of observing actual tests which sustained this view. Reinforcement can only be efficient in two ways, if the span both ways is the same or nearly so; a very little difference tends to throw the bulk of the load the short way, for stresses know only one law, namely, to follow the shortest line. In square panels the maximum bending comes on the mid-strips; those adjacent to the margin beams have very little bending parallel to the beam, practically all the action being the other way; and there are all gradations between. The reinforcing, therefore, should be spaced the

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Mr. Porter. minimum distance only in the mid-region, and from there on constantly widened, until, at about the quarter point, practically none is necessary, the slab arching across on the diagonal from beam to beam. The practice of spacing the bars at the minimum distance throughout is common, extending the bars to the very edge of the beams. In this case about half the steel is simply wasted.

In conclusion, the writer wishes to thank Mr. Godfrey for his very able paper, which to him has been exceedingly illuminative and fully appreciated, even though he has been obliged to differ from its contentions in some respects. On the other hand, perhaps, the writer is wrong and Mr. Godfrey right; in any event, if, through the medium of this contribution to the discussion, the writer has assisted in emphasizing a few of the fundamental truths; or if, in his points of nonconcordance, he is in coincidence with the views of a sufficient number of engineers to convince Mr. Godfrey of any mistaken stands; or, finally, if he has added anything new to the discussion which may help along the solution, he will feel amply repaid for his time and labor. The least that can be said is that reform all along the line, in matters of reinforced concrete design, is insistent.

Mr. Sewell. John Stephen Sewell, M. Am. Soc. C. E. (by letter).—The author is rather severe on the state of the art of designing reinforced concrete. It appears to the writer that, to a part of the indictment, at least, a plea of not guilty may properly be entered; and that some of the other charges may not be crimes, after all. There is still room for a wide difference of opinion on many points involved in the design of reinforced concrete, and too much zeal for conviction, combined with such skill in special pleading as this paper exhibits, may possibly serve to obscure the truth, rather than to bring it out clearly.

Point 1.—This is one to which the proper plea is "not guilty." The writer does not remember ever to have seen just the type of construction shown in Fig. 1, either used or recommended. The angle at which the bars are bent up is rarely as great as 45°, much less 60 degrees. The writer has never heard of "sharp bends" being insisted on, and has never seen them used; it is simply recommended or required that some of the bars be bent up and, in practice, the bend is always a gentle one. The stress to be carried by the concrete as a queen-post is never as great as that assumed by the author, and, in practice, the queen-post has a much greater bearing on the bars than is indicated in Fig. 1.

Point 2.—The writer, in a rather extensive experience, has never seen this point exemplified.

Point 3.—It is probable that as far as Point 3 relates to retaining walls, it touches a weak spot sometimes seen in actual practice, but necessity for adequate anchorage is discussed at great length in accepted literature, and the fault should be charged to the individual

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designer, for correct information has been within his reach for at least ten years. Mr. Sewell.

Point 4.—In this case it would seem that the author has put a wrong interpretation on what is generally meant by shear. However, it is undoubtedly true that actual shear in reinforcing steel is sometimes figured and relied on. Under some conditions it is good practice, and under others it is not. Transverse rods, properly placed, can surely act in transmitting stress from the stem to the flange of a T-beam, and could properly be so used. There are other conditions under which the concrete may hold the rods so rigidly that their shearing strength may be utilized; where such conditions do not obtain, it is not ordinarily necessary to count on the shearing strength of the rods.

Point 5.—Even if vertical stirrups do not act until the concrete has cracked, they are still desirable, as insuring a gradual failure and, generally, greater ultimate carrying capacity. It would seem that the point where their full strength should be developed is rather at the neutral axis than at the centroid of compression stresses. As they are usually quite light, this generally enables them to secure the requisite anchorage in the compressed part of the concrete. Applied to a riveted truss, the author's reasoning would require that all the rivets by which web members are attached to the top chord should be above the center of gravity of the chord section.

Point 6.—There are many engineers who, accepting the common theory of diagonal tension and compression in a solid beam, believe that, in a reinforced concrete beam with stirrups, the concrete can carry the diagonal compression, and the stirrups the tension. If these web stresses are adequately cared for, shear can be neglected.

The writer cannot escape the conclusion that tests which have been made support the above belief. He believes that stirrups should be inclined at an angle of 45° or less, and that they should be fastened rigidly to the horizontal bars; but that is merely the most efficient way to use them—not the only way to secure the desired action, at least, in some degree.

The author's proposed method of bending up some of the main bars is good, but he should not overlook the fact that he is taking them away from the bottom of the beam just as surely as in the case of a sharp bend, and this is one of his objections to the ordinary method of bending them up. Moreover, with long spans and varying distances of the load, the curve which he adopts for his bars cannot possibly be always the true equilibrium curve. His concrete must then act as a stiffening truss, and will almost inevitably crack before his cable can come into action as such.

Bulletin No. 29 of the University of Illinois contains nothing to indicate that the bars bent up in the tests reported were bent up in any other than the ordinary way; certainly they could not be con-

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Mr. Sewell. sidered as equivalent to the cables of a suspension bridge. These beams behaved pretty well, but the loads were applied so as to make them practically queen-post trusses, symmetrically loaded. While the bends in the bars were apparently not very sharp, and the angle of inclination was much less than 60°, or even 45°, it is not easy to find adequate bearings for the concrete posts on theoretical grounds, yet it is evident that the bearing was there just the same. The last four beams of the series, 521-1, 521-2, 521-5, 521-6, were about as nearly like Fig. 1 as anything the writer has ever seen in actual practice, yet they seem to have been the best of all. To be sure, the ends of the bent-up bars had a rather better anchorage, but they seem to have managed the shear question pretty much according to the expectation of their designer, and it is almost certain that the latter's assumptions would come under some part of the author's general indictment. These beams would seem to justify the art in certain practices condemned by the author. Perhaps he overlooked them.

Point 7.—The writer does not believe that the "general" practice as to continuity is on the basis charged. In fact, the general practice seems to him to be rather in the reverse direction. Personally, the writer believes in accepting continuity and designing for it, with moments at both center and supports equal to two-thirds of the center movement for a single span, uniformly loaded. He believes that the design of reinforced concrete should not be placed on the same footing as that of structural steel, because there is a fundamental difference, calling for different treatment. The basis should be sound, in both cases; but what is sound for one is not necessarily so for the other. In the author's plan for a series of spans designed as simple beams, with a reasonable amount of top reinforcement, he might get excessive stress and cracks in the concrete entirely outside of the supports. The shear would then become a serious matter, but no doubt the direct reinforcement would come into play as a suspension bridge, with further cracking of the concrete as a necessary preliminary.

Unfortunately, the writer is unable to refer to records, but he is quite sure that, in the early days, the rivets and bolts in the upper part of steel and iron bridge stringer connections gave some trouble by failing in tension due to continuous action, where the stringers were of moderate depth compared to the span. Possibly some members of the Society may know of such instances. The writer's instructors in structural design warned him against shallow stringers on that account, and told him that such things had happened.

Is it certain that structural steel design is on such a sound basis after all? Recent experiences seem to cast some doubt on it, and we may yet discover that we have escaped trouble, especially in buildings, because we almost invariably provide for loads much greater than are ever actually applied, and not because our knowledge and practice are especially exact.

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Point 8.—The writer believes that this point is well taken, as to a great deal of current practice; but, if the author's ideas are carried out, reinforced concrete will be limited to a narrow field of usefulness, because of weight and cost. With attached web members, the writer believes that steel can be concentrated in heavy members in a way that is not safe with plain bars, and that, in this way, much greater latitude of design may be safely allowed. Mr. Sewell.

Point 9.—The writer is largely in accord with the author's ideas on the subject of T-beams, but thinks he must have overlooked a very careful and able analysis of this kind of member, made by A. L. Johnson, M. Am. Soc. C. E., a number of years ago. While too much of the floor slab is still counted on for flange duty, it seems to the writer that, within the last few years, practice has greatly improved in this respect.

Point 10.—The author's statement regarding the beam and slab formulas in common use is well grounded. The modulus of elasticity of concrete is so variable that any formulas containing it and pretending to determine the stress in the concrete are unreliable, but the author's proposed method is equally so. We can determine by experiment limiting percentages of steel which a concrete of given quality can safely carry as reinforcement, and then use empirical formulas based on the stress in the steel and an assumed percentage of its depth in the concrete as a lever arm with more ease and just as much accuracy. The common methods result in designs which are safe enough, but they pretend to determine the stress in concrete; the writer does not believe that that is possible within 30% of the truth, and can see no profit in making laborious calculations leading to such unreliable results.

Point 11.—The writer has never designed a reinforced concrete chimney, but if he ever has to do so, he will surely not use any formula that is dependent on the modulus of elasticity of concrete.

Points 12, 13, and 14.—The writer has never had to consider these points to any extent in his own work, and will leave discussion to those better qualified.

Point 15.—There is much questionable practice in regard to reinforced concrete columns; but the matter is hardly disposed of as easily as indicated by the author. Other engineers draw different conclusions from the tests cited by the author, and from some to which he does not refer. To the writer it appears that here is a problem still awaiting solution on a really satisfactory basis. It seems incredible that the author would use plain concrete in columns, yet that seems to be the inference. The tests seem to indicate that there is much merit in both hooping and longitudinal reinforcement, if properly designed; that the fire-resisting covering should not be integral with the columns proper; that the high results obtained by M. Considère in testing small specimens cannot be depended on in practice, but that the

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Mr. Sewell.

reinforcement is of great value, nevertheless. The writer believes that when load-carrying capacity, stresses due to eccentricity, and fire-resisting qualities are all given due consideration, a type of column with close hooping and longitudinal reinforcement provided with shear members, will finally be developed, which will more than justify itself.

Point 16.—The writer has not gone as deeply into this question, from a theoretical point of view, as he would like; but he has had one experience that is pertinent. Some years ago, he built a plain slab floor supported by brick walls. The span was about 16 ft. The dimensions of the slab at right angles to the reinforcement was 100 ft. or more. Plain round bars, ½ in. in diameter, were run at right angles to the reinforcement about 2 ft. on centers, the object being to lessen cracks. The reinforcement consisted of Kahn bars, reaching from wall to wall. The rounds were laid on top of the Kahn bars. The concrete was frozen and undeniably damaged, but the floors stood up, without noticeable deflection, after the removal of the forms. The concrete was so soft, however, that a test was decided on. An area about 4 ft. wide, and extending to within about 1 ft. of each bearing wall, was loaded with bricks piled in small piers not in contact with each other, so as to constitute practically a uniformly distributed load. When the total load amounted to much less than the desired working load for the 4-ft. strip, considerable deflection had developed. As the load increased, the deflection increased, and extended for probably 15 or 20 ft. on either side of the loaded area. Finally, under about three-fourths of the desired breaking load for the 4-ft. strip, it became evident that collapse would soon occur. The load was left undisturbed and, in 3 or 4 min., an area about 16 ft. square tore loose from the remainder of the floor and fell. The first noticeable deflection in the above test extended for 8 or 10 ft. on either side of the loaded strip. It would seem that this test indicated considerable distributing power in the round rods, although they were not counted as reinforcement for load-carrying purposes at all. The concrete was extremely poor, and none of the steel was stressed beyond the elastic limit. While this test may not justify the designer in using lighter reinforcement for the short way of the slab, it at least indicates a very real value for some reinforcement in the other direction. It would seem to indicate, also, that light steel members in a concrete slab might resist a small amount of shear. The slab in this case was about 6 in. thick.

Mr. Thompson.

SANFORD E. THOMPSON, M. AM. SOC. C. E. (by letter).—Mr. Godfrey's sweeping condemnation of reinforced concrete columns, referred to in his fifteenth point, should not be passed over without serious criticism. The columns in a building, as he states, are the most vital portion of the structure, and for this very reason their design should be governed by theoretical and practical considerations based on the most comprehensive tests available.

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Mr. Thompson.

The quotation by Mr. Godfrey from a writer on hooped columns is certainly more radical than is endorsed by conservative engineers, but the best practice in column reinforcement, as recommended by the Joint Committee on Concrete and Reinforced Concrete, which assumes that the longitudinal bars assist in taking stress in accordance with the ratio of elasticity of steel to concrete, and that the hooping serves to increase the toughness of the column, is founded on the most substantial basis of theory and test.

In preparing the second edition of "Concrete, Plain and Reinforced," the writer examined critically the various tests of concrete columns in order to establish a definite basis for his conclusions. Referring more particularly to columns reinforced with vertical steel bars, an examination of all the tests of full-sized columns made in the United States appears to bear out the fact very clearly that longitudinal steel bars embedded in concrete increase the strength of the column, and, further, to confirm the theory by which the strength of the combination of steel and concrete may be computed and is computed in practice.

Tests of large columns have been made at the Watertown Arsenal, the Massachusetts Institute of Technology, the University of Illinois, by the City of Minneapolis, and at the University of Wisconsin. The results of these various tests were recently summarized by the writer in a paper presented at the January, 1910, meeting of the National Association of Cement Users.* Reference may be made to this paper for fuller particulars, but the averages of the tests of each series are worth repeating here.

In comparing the averages of reinforced columns, specimens with spiral or other hooping designed to increase the strength, or with horizontal reinforcement placed so closely together as to prevent proper placing of the concrete, are omitted. For the Watertown Arsenal tests the averages given are made up from fair representative tests on selected proportions of concrete, given in detail in the paper referred to, while in other cases all the corresponding specimens of the two types are averaged. The results are given in Table 1.

The comparison of these tests must be made, of course, independently in each series, because the materials and proportions of the concrete and the amounts of reinforcement are different in the different series. The averages are given simply to bring out the point, very definitely and distinctly, that longitudinally reinforced columns are stronger than columns of plain concrete.

A more careful analysis of the tests shows that the reinforced columns are not only stronger, but that the increase in strength due to the reinforcement averages greater than the ordinary theory, using a ratio of elasticity of 15, would predicate.

Certain of the results given are diametrically opposed to Mr. God-

* *Cement*, March, 1910, p. 343; and *Concrete Engineering*, May, 1910, p. 113.

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Mr. Thompson.

frey's conclusions from the same sets of tests. Reference is made by him, for example (page 69), to a plain column tested at the University of Illinois, which crushed at 2 004 lb. per sq. in., while a reinforced column of similar size crushed at 1 557 lb. per sq. in.,* and the author suggests that "This is not an isolated case, but appears to be the rule." Examination of this series of tests shows that it is somewhat more erratic than most of those made at the University of Illinois, but, even from the table referred to by Mr. Godfrey, pursuing his method of reasoning, the reverse conclusion might be reached, for if, instead of selecting, as he has done, the weakest reinforced column in the entire lot and the strongest plain column, a reverse selection had been made, the strength of the plain column would have been stated as 1 079 lb. per sq. in. and that of the reinforced column as 3 335 lb. per sq. in. If extremes are to be selected at all, the weakest reinforced column should be compared with the weakest plain column, and the strongest reinforced column with the strongest plain column; and the results would show that while an occasional reinforced column may be low in strength, an occasional plain column will be still lower, so that the reinforcement, even by this comparison, is of marked advantage in increasing strength. In such cases, however, comparisons should be made by averages. The average strength of the reinforced columns, even in this series, as given in Table 1, is considerably higher than that of the plain columns.

TABLE 1.—AVERAGE RESULTS OF TESTS OF PLAIN *vs.* LONGITUDINALLY REINFORCED COLUMNS.

Location of test.	Average strength of plain columns.	Average strength of longitudinally reinforced columns.	Reference.
Watertown Arsenal.....	1 781	2 992	Taylor and Thompson's "Concrete, Plain and Reinforced" (2d edition), p. 498.
Massachusetts Institute of Technology.......	1 750	2 370	*Transactions*, Am. Soc. C. E., Vol. L, p. 487.
University of Illinois...	1 550	1 750	*Bulletin No. 10*, University of Illinois, 1907.
City of Minneapolis.....	2 020	2 300	*Engineering News*, Dec. 3d, 1908, p. 608.
University of Wisconsin.	2 033	2 438	*Proceedings*, Am. Soc. for Testing Materials, Vol. IX, 1909, p. 477.

In referring, in the next paragraph, to Mr. Withey's tests at the University of Wisconsin, Mr. Godfrey selects for his comparison two groups of concrete which are not comparable. Mr. Withey, in the paper describing the tests, refers to two groups of plain concrete

* The correct figures from the *Bulletin* are 1 577 lb.

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columns, A 1 to A 4, and W 1 to W 3. He speaks of the uniformity in the tests of the former group, the maximum variation in the four specimens being only 2%, but states, with reference to columns, W 1 to W 3, that: Mr. Thompson.

"As these 3 columns were made of a concrete much superior to that in any of the other columns made from 1:2:4 or 1:2:3½ mix, they cannot satisfactorily be compared with them. Failures of all plain columns were sudden and without any warning."

Now, Mr. Godfrey, instead of taking columns A 1 to A 3, selects for his comparison W 1 to W 3, made, as Mr. Withey distinctly states, with an especially superior concrete. Taking columns, A 1 to A 3, for comparison with the reinforced columns, E 1 to E 3, the result shows an average of 2 033 for the plain columns and 2 438 for the reinforced columns.

Again, taking the third series of tests referred to by Mr. Godfrey, those at Minneapolis, Minn., it is to be noticed that he selects for his criticism a column which has this note as to the manner of failure: "Bending at center (bad batch of concrete at this point)." Furthermore, the column is only 9 by 9 in., and square, and the stress referred to is calculated on the full section of the column instead of on the strength within the hooping, although the latter method is the general practice in a hooped column. The inaccuracy of this is shown by the fact that, with this small size of square column, more than half the area is outside the hooping and never taken into account in theoretical computations. A fair comparison, as far as longitudinal reinforcement is concerned, is always between the two plain columns and the six columns, E, D, and F. The results are so instructive that a letter* by the writer is quoted in full as follows:

"SIR:—

"In view of the fact that the column tests at Minneapolis, as reported in your paper of December 3, 1908, p. 608, are liable because of the small size of the specimens to lead to divergent conclusions, a few remarks with reference to them may not be out of place at this time.

"1. It is evident that the columns were all smaller, being only 9 in. square, than is considered good practice in practical construction, because of the difficulty of properly placing the concrete around the reinforcement.

"2. The tests of columns with flat bands, A, B, and C, in comparison with the columns E, D and F, indicate that the wide bands affected the placing of the concrete, separating the internal core from the outside shell so that it would have been nearly as accurate to base the strength upon the material within the bands, that is, upon a section of 38 sq. in., instead of upon the total area of 81 sq. in. This set of tests, A, B and C, is therefore inconclusive except as showing the practical difficulty in the use of bands in small columns, and the

* *Engineering News*, January 7th, 1909, p. 20.

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Mr. Thompson.

necessity for disregarding all concrete outside of the bands when computing the strength.

"3. The six columns *E, D* and *F,* each of which contained eight ⅝-in. rods, are the only ones which are a fair test of columns longitudinally reinforced, since they are the only specimens except the plain columns in which the small sectional area was not cut by bands or hoops. Taking these columns, we find an average strength 38% in excess of the plain columns, whereas, with the percentage of reinforcement used, the ordinary formula for vertical steel (using a ratio of elasticity of steel to concrete of 15) gives 34% as the increase which might be expected. In other words, the actual strength of this set of columns was in excess of the theoretical strength. The wire bands on these columns could not be considered even by the advocates of hooped columns as appreciably adding to the strength, because they were square instead of circular. It may be noted further in connection with these longitudinally reinforced columns that the results were very uniform and, further, that the strength of *every specimen* was much greater than the strength of the plain columns, being in every case except one at least 40% greater. In these columns the rods buckled between the bands, but they evidently did not do so until their elastic limit was passed, at which time of course they would be expected to fail.

"4. With reference to columns, *A, B, C* and *L,* which were essentially hooped columns, the failure appears to have been caused by the greater deformation which is always found in hooped columns, and which in the earlier stages of the loading is apparently due to lack of homogeneity caused by the difficulty in placing the concrete around the hooping, and in the later stage of the loading to the excessive expansion of the concrete. This greater deformation in a hooped column causes any vertical steel to pass its elastic limit at an earlier stage than in a column where the deformation is less, and therefore produces the buckling between the bands which is noted in these two sets of columns. This excessive deformation is a strong argument against the use of high working stresses in hooped columns.

"In conclusion, then, it may be said that the columns reinforced with vertical round rods showed all the strength that would be expected of them by theoretical computation. The hooped columns, on the other hand, that is, the columns reinforced with circular bands and hoops, gave in all cases comparatively low results, but no conclusions can be drawn from them because the unit-strength would have been greatly increased if the columns had been larger so that the relative area of the internal core to the total area of the column had been greater."

From this letter, it will be seen that every one of Mr. Godfrey's comparisons of plain *versus* reinforced columns requires explanations which decidedly reduce, if they do not entirely destroy, the force of his criticism.

This discussion can scarcely be considered complete without brief reference to the theory of longitudinal steel reinforcement for columns. The principle* is comparatively simple. When a load is placed on a

* For fuller treatment, see the writer's discussion in *Transactions*, Am. Soc. C. E., Vol. LXI, p. 46.

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column of any material it is shortened in proportion, within working limits, to the load placed upon it; that is, with a column of homogeneous material, if the load is doubled, the amount of shortening or deformation is also doubled. If vertical steel bars are embedded in concrete, they must shorten when the load is applied, and consequently relieve the concrete of a portion of its load. It is therefore physically impossible to prevent such vertical steel from taking a portion of the load unless the steel slips or buckles. Mr. Thompson.

As to the possible danger of the bars in the concrete slipping or buckling, to which Mr. Godfrey also refers, again must tests be cited. If the ends are securely held—and this is always the case when bars are properly butted or are lapped for a sufficient length—they cannot slip. With reference to buckling, tests have proved conclusively that vertical bars such as are used in columns, when embedded in concrete, will not buckle until the elastic limit of the steel is reached, or until the concrete actually crushes. Beyond these points, of course, neither steel nor concrete nor any other material is expected to do service.

As proof of this statement, it will be seen, by reference to tests at the Watertown Arsenal, as recorded in "Tests of Metals," that many of the columns were made with vertical bar reinforcement having absolutely no hoops or horizontal steel placed around them. That is, the bars, 8 ft. long, were placed in the four corners of the column—in some tests only 2 in. from the surface—and held in place simply by the concrete itself.* There was no sign whatever of buckling until the compression was so great that the elastic limit of the steel was passed, when, of course, no further strength could be expected from it.

To recapitulate the conclusions reached as a result of a study of the tests: It is evident that, not only does theory permit the use of longitudinal bar reinforcement for increasing the strength of concrete columns, whenever such reinforcement is considered advisable, but that all the important series of column tests made in the United States to date show a decisive increase in strength of columns reinforced with longitudinal steel bars over those which are not reinforced. Furthermore, as has already been mentioned, without treating the details of the proof, it can be shown that the tests bear out conclusively the conservatism of computing the value of the vertical steel bars in compression by the ordinary formulas based on the ratio of the moduli of elasticity of steel to concrete.

EDWARD GODFREY, M. AM. SOC. C. E. (by letter).—As was to be expected, this paper has brought out discussion, some of which is favorable and flattering; some is in the nature of dust-throwing to obscure the force of the points made; some would attempt to belittle the importance of these points; and some simply brings out the old Mr. Godfrey.

* See "Tests of Metals," U. S. A., 1905, p. 344.

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Mr. Godfrey.

and over-worked argument which can be paraphrased about as follows: "The structures stand up and perform their duty, is this not enough?"

The last-mentioned argument is as old as Engineering; it is the "practical man's" mainstay, his "unanswerable argument." The so-called practical man will construct a building, and test it either with loads or by practical use. Then he will modify the design somewhere, and the resulting construction will be tested. If it passes through this modifying process and still does service, he has something which, in his mind, is unassailable. Imagine the freaks which would be erected in the iron bridge line, if the capacity to stand up were all the designer had to guide him, analysis of stresses being unknown. Tests are essential, but analysis is just as essential. The fact that a structure carries the bare load for which it is computed, is in no sense a test of its correct design; it is not even a test of its safety. In Pittsburg, some years ago, a plate-girder span collapsed under the weight of a locomotive which it had carried many times. This bridge was, perhaps, thirty years old. Some reinforced concrete bridges have failed under loads which they have carried many times. Others have fallen under no extraneous load, and after being in service many months. If a large number of the columns of a structure fall shortly after the forms are removed, what is the factor of safety of the remainder, which are identical, but have not quite reached their limit of strength? Or what is the factor of safety of columns in other buildings in which the concrete was a little better or the forms have been left in a little longer, both sets of columns being similarly designed?

There are highway bridges of moderately long spans standing and doing service, which have 2-in. chord pins; laterals attached to swinging floor-beams in such a way that they could not possibly receive their full stress; eye-bars with welded-on heads; and many other equally absurd and foolish details, some of which were no doubt patented in their day. Would any engineer with any knowledge whatever of bridge design accept such details? They often stand the test of actual service for years; in pins, particularly, the calculated stress is sometimes very great. These details do not stand the test of analysis and of common sense, and, therefore, no reputable engineer would accept them.

Mr. Turner, in the first and second paragraphs of his discussion, would convey the impression that the writer was in doubt as to his "personal opinions" and wanted some free advice. He intimates that he is too busy to go fully into a treatise in order to set them right. He further tries to throw discredit on the paper by saying that the writer has adduced no clean-cut statement of fact or tests in support of his views. If Mr. Turner had read the paper carefully, he would not have had the idea that in it the hooped column is condemned. As

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to this more will be said later. The paper is simply and solely a collection of statements of facts and tests, whereas his discussion teems with his "personal opinion," and such statements as "These values * * * are regarded by the writer as having at least double the factor of safety used in ordinary designs of structural steel"; "On a basis not far from that which the writer considers reasonable practice." Do these sound like clean-cut statements of fact, or are they personal opinions? It is a fact, pure and simple, that a sharp bend in a reinforcing rod in concrete violates the simplest principles of mechanics; also that the queen-post and Pratt and Howe truss analogies applied to reinforcing steel in concrete are fallacies; that a few inches of embedment will not anchor a rod for its value; that concrete shrinks in setting in air and puts initial stress in both the concrete and the steel, making assumed unstressed initial conditions non-existent. It is a fact that longitudinal rods alone cannot be relied on to reinforce a concrete column. Contrary to Mr. Turner's statement, tests have been adduced to demonstrate this fact. Further, it is a fact that the faults and errors in reinforced concrete design to which attention is called, are very common in current design, and are held up as models in nearly all books on the subject. Mr. Godfrey.

The writer has not asked any one to believe a single thing because he thinks it is so, or to change a single feature of design because in his judgment that feature is faulty. The facts given are exemplifications of elementary mechanical principles overlooked by other writers, just as early bridge designers and writers on bridge design overlooked the importance of calculating bridge pins and other details which would carry the stress of the members.

A careful reading of the paper will show that the writer does not accept the opinions of others, when they are not backed by sound reason, and does not urge his own opinion.

Instead of being a statement of personal opinion for which confirmation is desired, the paper is a simple statement of facts and tests which demonstrate the error of practices exhibited in a large majority of reinforced concrete work and held up in the literature on the subject as examples to follow. Mr. Turner has made no attempt to deny or refute any one of these facts, but he speaks of the burden of proof resting on the writer. Further, he makes statements which show that he fails entirely to understand the facts given or to grasp their meaning. He says that the writer's idea is "that the entire pull of the main reinforcing rod should be taken up apparently at the end." He adds that the soundness of this position may be questioned, because, in slabs, the steel frequently breaks at the center. Compare this with the writer's statement, as follows:

"In shallow beams there is little need of provision for taking shear by any other means than the concrete itself. The writer has seen

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Mr. Godfrey. a reinforced slab support a very heavy load by simple friction, for the slab was cracked close to the supports. In slabs, shear is seldom provided for in the steel reinforcement. It is only when beams begin to have a depth approximating one-tenth of the span that the shear in the concrete becomes excessive and provision is necessary in the steel reinforcement. Years ago, the writer recommended that, in such beams, some of the rods be curved up toward the ends of the span and anchored over the support."

It is solely in providing for shear that the steel reinforcement should be anchored for its full value over the support. The shear must ultimately reach the support, and that part which the concrete is not capable of carrying should be taken to it solely by the steel, as far as tensile and shear stresses are concerned. It should not be thrown back on the concrete again, as a system of stirrups must necessarily do.

The following is another loose assertion by Mr. Turner:

"Mr. Godfrey appears to consider that the hooping and vertical reinforcement of columns is of little value. He, however, presents for consideration nothing but his opinion of the matter, which appears to be based on an almost total lack of familiarity with such construction."

There is no excuse for statements like this. If Mr. Turner did not read the paper, he should not have attempted to criticize it. What the writer presented for consideration was more than his opinion of the matter. In fact, no opinion at all was presented. What was presented was tests which prove absolutely that longitudinal rods without hoops may actually reduce the strength of a column, and that a column containing longitudinal rods and "hoops which are not close enough to stiffen the rods" may be of less strength than a plain concrete column. A properly hooped column was not mentioned, except by inference, in the quotation given in the foregoing sentence. The column tests which Mr. Turner presents have no bearing whatever on the paper, for they relate to columns with bands and close spirals. Columns are sometimes built like these, but there is a vast amount of work in which hooping and bands are omitted or are reduced to a practical nullity by being spaced a foot or so apart.

A steel column made up of several pieces latticed together derives a large part of its stiffness and ability to carry compressive stresses from the latticing, which should be of a strength commensurate with the size of the column. If it were weak, the column would suffer in strength. The latticing might be very much stronger than necessary, but it would not add anything to the strength of the column to resist compression. A formula for the compressive strength of a column could not include an element varying with the size of the lattice. If the lattice is weak, the column is simply deficient; so a formula for a hooped column is incorrect if it shows that the strength of the column varies with the section of the hoops, and, on this account, the common formula is incorrect. The hoops might be ever so strong,

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Mr. Godfrey.

beyond a certain limit, and yet not an iota would be added to the compressive strength of the column, for the concrete between the hoops might crush long before their full strength was brought into play. Also, the hoops might be too far apart to be of much or any benefit, just as the lattice in a steel column might be too widely spaced. There is no element of personal opinion in these matters. They are simply incontrovertible facts. The strength of a hooped column, disregarding for the time the longitudinal steel, is dependent on the fact that thin discs of concrete are capable of carrying much more load than shafts or cubes. The hoops divide the column into thin discs, if they are closely spaced; widely spaced hoops do not effect this. Thin joints of lime mortar are known to be many times stronger than the same mortar in cubes. Why, in the many books on the subject of reinforced concrete, is there no mention of this simple principle? Why do writers on this subject practically ignore the importance of toughness or tensile strength in columns? The trouble seems to be in the tendency to interpret concrete in terms of steel. Steel at failure in short blocks will begin to spread and flow, and a short column has nearly the same unit strength as a short block. The action of concrete under compression is quite different, because of the weakness of concrete in tension. The concrete spalls off or cracks apart and does not flow under compression, and the unit strength of a shaft of concrete under compression has little relation to that of a flat block. Some years ago the writer pointed out that the weakness of cast-iron columns in compression is due to the lack of tensile strength or toughness in cast iron. Compare 7 600 lb. per sq. in. as the base of a column formula for cast iron with 100 000 lb. per sq. in. as the compressive strength of short blocks of cast iron. Then compare 750 lb. per sq. in., sometimes used in concrete columns, with 2 000 lb. per sq. in., the ultimate strength in blocks. A material one-fiftieth as strong in compression and one-hundredth as strong in tension with a "safe" unit one-tenth as great! The greater tensile strength of rich mixtures of concrete accounts fully for the greater showing in compression in tests of columns of such mixtures. A few weeks ago, an investigator in this line remarked, in a discussion at a meeting of engineers, that "the failure of concrete in compression may in cases be due to lack of tensile strength." This remark was considered of sufficient novelty and importance by an engineering periodical to make a special news item of it. This is a good illustration of the state of knowledge of the elementary principles in this branch of engineering.

Mr. Turner states, "Again, concrete is a material which shows to the best advantage as a monolith, and, as such, the simple beam seems to be decidedly out of date to the experienced constructor." Similar things could be said of steelwork, and with more force. Riveted trusses are preferable to articulated ones for rigidity. The stringers

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Mr. Godfrey. of a bridge could readily be made continuous; in fact, the very riveting of the ends to a floor-beam gives them a large capacity to carry reverse moments. This strength is frequently taken advantage of at the end floor-beam, where a tie is made to rest on a bracket having the same riveted connection as the stringer. A small splice-plate across the top flanges of the stringers would greatly increase this strength to resist reverse moments. A steel truss span is ideally conditioned for continuity in the stringers, since the various supports are practically relatively immovable. This is not true in a reinforced concrete building where each support may settle independently and entirely vitiate calculated continuous stresses. Bridge engineers ignore continuity absolutely in calculating the stringers; they do not argue that a simple beam is out of date. Reinforced concrete engineers would do vastly better work if they would do likewise, adding top reinforcement over supports to forestall cracking only. Failure could not occur in a system of beams properly designed as simple spans, even if the negative moments over the supports exceeded those for which the steel reinforcement was provided, for the reason that the deflection or curving over the supports can only be a small amount, and the simple-beam reinforcement will immediately come into play.

Mr. Turner speaks of the absurdity of any method of calculating a multiple-way reinforcement in slabs by endeavoring to separate the construction into elementary beam strips, referring, of course, to the writer's method. This is misleading. The writer does not endeavor to "separate the construction into elementary beam strips" in the sense of disregarding the effect of cross-strips. The "separation" is analogous to that of considering the tension and compression portions of a beam separately in proportioning their size or reinforcement, but unitedly in calculating their moment. As stated in the paper, "strips are taken across the slab and the moment in them is found, considering the limitations of the several strips in deflection imposed by those running at right angles therewith." It is a sound and rational assumption that each strip, 1 ft. wide through the middle of the slab, carries its half of the middle square foot of the slab load. It is a necessary limitation that the other strips which intersect one of these critical strips across the middle of the slab, cannot carry half of the intercepted square foot, because the deflection of these other strips must diminish to zero as they approach the side of the rectangle. Thus, the nearer the support a strip parallel to that support is located, the less load it can take, for the reason that it cannot deflect as much as the middle strip. In the oblong slab the condition imposed is equal deflection of two strips of unequal span intersecting at the middle of the slab, as well as diminished deflection of the parallel strips.

In this method of treating the rectangular slab, the concrete in tension is not considered to be of any value, as is the case in all accepted methods.

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Some years ago the writer tested a number of slabs in a building, with a load of 250 lb. per sq. ft. These slabs were 3 in. thick and had a clear span of 44 in. between beams. They were totally without reinforcement. Some had cracked from shrinkage, the cracks running through them and practically the full length of the beams. They all carried this load without any apparent distress. If these slabs had been reinforced with some special reinforcement of very small cross-section, the strength which was manifestly in the concrete itself, might have been made to appear to be in the reinforcement. Magic properties could be thus conjured up for some special brand of reinforcement. An energetic proprietor could capitalize tension in concrete in this way and "prove" by tests his claims to the magic properties of his reinforcement. Mr. Godfrey.

To say that Poisson's ratio has anything to do with the reinforcement of a slab is to consider the tensile strength of concrete as having a positive value in the bottom of that slab. It means to reinforce for the stretch in the concrete and not for the tensile stress. If the tensile strength of concrete is not accepted as an element in the strength of a slab having one-way reinforcement, why should it be accepted in one having reinforcement in two or more directions? The tensile strength of concrete in a slab of any kind is of course real, when the slab is without cracks; it has a large influence in the deflection; but what about a slab that is cracked from shrinkage or otherwise?

Mr. Turner dodges the issue in the matter of stirrups by stating that they were not correctly placed in the tests made at the University of Illinois. He cites the Hennebique system as a correct sample. This system, as the writer finds it, has some rods bent up toward the support and anchored over it to some extent, or run into the next span. Then stirrups are added. There could be no objection to stirrups if, apart from them, the construction were made adequate, except that expense is added thereby. Mr. Turner cannot deny that stirrups are very commonly used just as they were placed in the tests made at the University of Illinois. It is the common practice and the prevailing logic in the literature of the subject which the writer condemns.

Mr. Thacher says of the first point:

"At the point where the first rod is bent up, the stress in this rod runs out. The other rods are sufficient to take the horizontal stress, and the bent-up portion provides only for the vertical and diagonal shearing stresses in the concrete."

If the stress runs out, by what does that rod, in the bent portion, take shear? Could it be severed at the bend, and still perform its office? The writer can conceive of an inclined rod taking the shear of a beam if it were anchored at each end, or long enough somehow to have a grip in the concrete from the centroid of compression up

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Mr. Godfrey. and from the center of the steel down. This latter is a practical impossibility. A rod curved up from the bottom reinforcement and curved to a horizontal position and run to the support with anchorage, would take the shear of a beam. As to the stress running out of a rod at the point where it is bent up, this will hardly stand the test of analysis in the majority of cases. On account of the parabolic variation of stress in a beam, there should be double the length necessary for the full grip of a rod in the space from the center to the end of a beam. If 50 diameters are needed for this grip, the whole span should then be not less than four times 50, or 200 diameters of the rod. For the same reason the rod between these bends should be at least 200 diameters in length. Often the reinforcing rods are equal to or more than one-two-hundredth of the span in diameter, and therefore need the full length of the span for grip.

Mr. Thacher states that Rod 3 provides for the shear. He fails to answer the argument that this rod is not anchored over the support to take the shear. Would he, in a queen-post truss, attach the hog-rod to the beam some distance out from the support and thus throw the bending and shear back into the very beam which this rod is intended to relieve of bending and shear? Yet this is just what Rod 3 would do, if it were long enough to be anchored for the shear, which it seldom is; hence it cannot even perform this function. If Rod 3 takes the shear, it must give it back to the concrete beam from the point of its full usefulness to the support. Mr. Thacher would not say of a steel truss that the diagonal bars would take the shear, if these bars, in a deck truss, were attached to the top chord several feet away from the support, or if the end connection were good for only a fraction of the stress in the bars. Why does he not apply the same logic to reinforced concrete design?

Answering the third point, Mr. Thacher makes more statements that are characteristic of current logic in reinforced concrete literature, which does not bother with premises. He says, "In a beam, the shear rods run through the compression parts of the concrete and have sufficient anchorage." If the rods have sufficient anchorage, what is the nature of that anchorage? It ought to be possible to analyze it, and it is due to the seeker after truth to produce some sort of analysis. What mysterious thing is there to anchor these rods? The writer has shown by analysis that they are not anchored sufficiently. In many cases they are not long enough to receive full anchorage. Mr. Thacher merely makes the dogmatic statement that they are anchored. There is a faint hint of a reason in his statement that they run into the compression part of the concrete. Does he mean that the compression part of the concrete will grip the rod like a vise? How does this comport with his contention farther on that the beams are continuous? This would mean tension in the upper part of the beam. In any beam

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the compression near the support, where the shear is greatest, is small; so even this hint of an argument has no force or meaning. Mr. Godfrey.

In this same paragraph Mr. Thacher states, concerning the third point and the case of the retaining wall that is given as an example, "In a counterfort, the inclined rods are sufficient to take the overturning stress." Mr. Thacher does not make clear what he means by "overturning stress." He seems to mean the force tending to pull the counterfort loose from the horizontal slab. The weight of the earth fill over this slab is the force against which the vertical and inclined rods of Fig. 2, at *a*, must act. Does Mr. Thacher mean to state seriously that it is sufficient to hang this slab, with its heavy load of earth fill, on the short projecting ends of a few rods? Would he hang a floor slab on a few rods which project from the bottom of a girder? He says, "The proposed method is no more effective." The proposed method is Fig. 2, at *b*, where an angle is provided as a shelf on which this slab rests. The angle is supported, with thread and nut, on rods which reach up to the front slab, from which a horizontal force, acting about the toe of the wall as a fulcrum, results in the lifting force on the slab. There is positively no way in which this wall could fail (as far as the counterfort is concerned) but by the pulling apart of the rods or the tearing out of this anchoring angle. Compare this method of failure with the mere pulling out of a few ends of rods, in the design which Mr. Thacher says is just as effective. This is another example of the kind of logic that is brought into requisition in order to justify absurd systems of design.

Mr. Thacher states that shear would govern in a bridge pin where there is a wide bar or bolster or a similar condition. The writer takes issue with him in this. While in such a case the center of bearing need not be taken to find the bending moment, shear would not be the correct governing element. There is no reason why a wide bar or a wide bolster should take a smaller pin than a narrow one, simply because the rule that uses the center of bearing would give too large a pin. Bending can be taken in this, as in other cases, with a reasonable assumption for a proper bearing depth in the wide bar or bolster. The rest of Mr. Thacher's comment on the fourth point avoids the issue. What does he mean by "stress" in a shear rod? Is it shear or tension? Mr. Thacher's statement, that the "stress" in the shear rods is less than that in the bottom bars, comes close to saying that it is shear, as the shearing unit in steel is less than the tensile unit. This vague way of referring to the "stress" in a shear member, without specifically stating whether this "stress" is shear or tension, as was done in the Joint Committee Report, is, in itself, a confession of the impossibility of analyzing the "stress" in these members. It gives the designer the option of using tension or shear, both of which are absurd in the ordinary method of design. Writers of books are

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Mr. Godfrey. not bold enough, as a rule, to state that these rods are in shear, and yet their writings are so indefinite as to allow this very interpretation.

Mr. Thacher criticises the fifth point as follows:

"Vertical stirrups are designed to act like the vertical rods in a Howe truss. Special literature is not required on the subject; it is known that the method used gives good results, and that is sufficient."

This is another example of the logic applied to reinforced concrete design—another dogmatic statement. If these stirrups act like the verticals in a Howe truss, why is it not possible by analysis to show that they do? Of course there is no need of special literature on the subject, if it is the intention to perpetuate this senseless method of design. No amount of literature can prove that these stirrups act as the verticals of a Howe truss, for the simple reason that it can be easily proven that they do not.

Mr. Thacher's criticism of the sixth point is not clear. "All the shear from the center of the beam up to the bar in question," is what he says each shear member is designed to take in the common method. The shear of a beam usually means the sum of the vertical forces in a vertical section. If he means that the amount of this shear is the load from the center of the beam to the bar in question, and that shear members are designed to take this amount of shear, it would be interesting to know by what interpretation the common method can be made to mean this. The method referred to is that given in several standard works and in the Joint Committee Report. The formula in that report for vertical reinforcement is:

$$P = \frac{V\,s}{j\,d},$$

in which P = the stress in a single reinforcing member, V = the proportion of total shear assumed as carried by the reinforcement, s = the horizontal spacing of the reinforcing members, and $j\,d$ = the effective depth.

Suppose the spacing of shear members is one-half or one-third of the effective depth, the stress in each member is one-half or one-third of the "shear assumed to be carried by the reinforcement." Can Mr. Thacher make anything else out of it? If, as he says, vertical stirrups are designed to act like the vertical rods in a Howe truss, why are they not given the stress of the verticals of a Howe truss instead of one-half or one-third or a less proportion of that stress?

Without meaning to criticize the tests made by Mr. Thaddeus Hyatt on curved-up rods with nuts and washers, it is true that the results of many early tests on reinforced concrete are uncertain, because of the mealy character of the concrete made in the days when "a minimum amount of water" was the rule. Reinforcement slips in such concrete when it would be firmly gripped in wet concrete. The writer has been unable to find any record of the tests to which

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Mr. Thacher refers. The tests made at the University of Illinois, far from showing reinforcement of this type to be "worse than useless," showed most excellent results by its use. Mr. Godfrey.

That which is condemned in the seventh point is not so much the calculating of reinforced concrete beams as continuous, and reinforcing them properly for these moments, but the common practice of lopping off arbitrarily a large fraction of the simple beam moment on reinforced concrete beams of all kinds. This is commonly justified by some virtue which lies in the term monolith. If a beam rests in a wall, it is "fixed ended"; if it comes into the side of a girder, it is "fixed ended"; and if it comes into the side of a column, it is the same. This is used to reduce the moment at mid-span, but reinforcement which will make the beam fixed ended or continuous is rare.

There is not much room for objection to Mr. Thacher's rule of spacing rods three diameters apart. The rule to which the writer referred as being 66% in error on the very premise on which it was derived, namely, shear equal to adhesion, was worked out by F. P. McKibben, M. Am. Soc. C. E. It was used, with due credit, by Messrs. Taylor and Thompson in their book, and, without credit, by Professors Maurer and Turneaure in their book. Thus five authorities perpetrate an error in the solution of one of the simplest problems imaginable. If one author of an arithmetic had said two twos are five, and four others had repeated the same thing, would it not show that both revision and care were badly needed?

Ernest McCullough, M. Am. Soc. C. E., in a paper read at the Armour Institute, in November, 1908, says, "If the slab is not less than one-fifth of the total depth of the beam assumed, we can make a T-section of it by having the narrow stem just wide enough to contain the steel." This partly answers Mr. Thacher's criticism of the ninth point. In the next paragraph, Mr. McCullough mentions some very nice formulas for T-beams by a certain authority. Of course it would be better to use these nice formulas than to pay attention to such "rule-of-thumb" methods as would require more width in the stem of the T than enough to squeeze the steel in.

If these complex formulas for T-beams (which disregard utterly the simple and essential requirement that there must be concrete enough in the stem of the T to grip the steel) are the only proper exemplifications of the "theory of T-beams," it is time for engineers to ignore theory and resort to rule-of-thumb. It is not theory, however, which is condemned in the paper, it is complex theory; theory totally out of harmony with the materials dealt with; theory based on false assumptions; theory which ignores essentials and magnifies trifles; theory which, applied to structures which have failed from their own weight, shows them to be perfectly safe and correct in design; half-baked theories which arrogate to themselves a monopoly on rationality.

To return to the spacing of rods in the bottom of a T-beam; the

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Mr. Godfrey. report of the Joint Committee advocates a horizontal spacing of two and one-half diameters and a side spacing of two diameters to the surface. The same report advocates a "clear spacing between two layers of bars of not less than ½ in." Take a T-beam, 11½ in. wide, with two layers of rods 1 in. square, 4 in each layer. The upper surface of the upper layer would be 3½ in. above the bottom of the beam. Below this surface there would be 32 sq. in. of concrete to grip 8 sq. in. of steel. Does any one seriously contend that this trifling amount of concrete will grip this large steel area? This is not an extreme case; it is all too common; and it satisfies the requirements of the Joint Committee, which includes in its make-up a large number of the best-known authorities in the United States.

Mr. Thacher says that the writer appears to consider theories for reinforced concrete beams and slabs as useless refinements. This is not what the writer intended to show. He meant rather that facts and tests demonstrate that refinement in reinforced concrete theories is utterly meaningless. Of course a wonderful agreement between the double-refined theory and test can generally be effected by "hunching" the modulus of elasticity to suit. It works both ways, the modulus of elasticity of concrete being elastic enough to be shifted again to suit the designer's notion in selecting his reinforcement. All of which is very beautiful, but it renders standard design impossible.

Mr. Thacher characterizes the writer's method of calculating reinforced concrete chimneys as rule-of-thumb. This is surprising after what he says of the methods of designing stirrups. The writer's method would provide rods to take all the tensile stresses shown to exist by any analysis; it would give these rods unassailable end anchorages; every detail would be amply cared for. If loose methods are good enough for proportioning loose stirrups, and no literature is needed to show why or how they can be, why analyze a chimney so accurately and apply assumptions which cannot possibly be realized anywhere but on paper and in books?

It is not rule-of-thumb to find the tension in plain concrete and then embed steel in that concrete to take that tension. Moreover, it is safer than the so-called rational formula, which allows compression on slender rods in concrete.

Mr. Thacher says, "No arch designed by the elastic theory was ever known to fail, unless on account of insecure foundations." Is this the correct way to reach correct methods of design? Should engineers use a certain method until failures show that something is wrong? It is doubtful if any one on earth has statistics sufficient to state with any authority what is quoted in the opening sentence of this paragraph. Many arches are failures by reason of cracks, and these cracks are not always due to insecure foundations. If Mr. Thacher means by insecure foundations, those which settle, his assertion, assuming it to be true,

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has but little weight. It is not always possible to found an arch on rock. Some settlement may be anticipated in almost every foundation. As commonly applied, the elastic theory is based on the absolute fixity of the abutments, and the arch ring is made more slender because of this fixity. The ordinary "row-of-blocks" method gives a stiffer arch ring and, consequently, greater security against settlement of foundations. Mr. Godfrey.

In 1904, two arches failed in Germany. They were three-hinged masonry arches with metal hinges. They appear to have gone down under the weight of theory. If they had been made of stone blocks in the old-fashioned way, and had been calculated in the old-fashioned row-of-blocks method, a large amount of money would have been saved. There is no good reason why an arch cannot be calculated as hinged ended and built with the arch ring anchored into the abutments. The method of the equilibrium polygon is a safe, sane, and sound way to calculate an arch. The monolithic method is a safe, sane, and sound way to build one. People who spend money for arches do not care whether or not the fancy and fancied stresses of the mathematician are realized; they want a safe and lasting structure.

Of course, calculations can be made for shrinkage stresses and for temperature stresses. They have about as much real meaning as calculations for earth pressures behind a retaining wall. The danger does not lie in making the calculations, but in the confidence which the very making of them begets in their correctness. Based on such confidence, factors of safety are sometimes worked out to the hundredth of a unit.

Mr. Thacher is quite right in his assertion that stiff steel angles, securely latticed together, and embedded in the concrete column, will greatly increase its strength.

The theory of slabs supported on four sides is commonly accepted for about the same reason as some other things. One author gives it, then another copies it; then when several books have it, it becomes authoritative. The theory found in most books and reports has no correct basis. That worked out by Professor W. C. Unwin, to which the writer referred, was shown by him to be wrong.* An important English report gave publicity and much space to this erroneous solution. Messrs. Marsh and Dunn, in their book on reinforced concrete, give several pages to it.

In referring to the effect of initial stress, Mr. Myers cites the case of blocks and says, "Whatever initial stress exists in the concrete due to this process of setting exists also in these blocks when they are tested." However, the presence of steel in beams and columns puts internal stresses in reinforced concrete, which do not exist in an isolated block of plain concrete.

* *The Engineering Record*, August 17th, 1907.

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Mr. Godfrey.

Mr. Meem, while he states that he disagrees with the writer in one essential point, says of that point, "In the ordinary way in which these rods are used, they have no practical value." The paper is meant to be a criticism of the ordinary way in which reinforced concrete is used.

While Mr. Meem's formula for a reinforced concrete beam is simple and much like that which the writer would use, he errs in making the moment of the stress in the steel about the neutral axis equal to the moment of that in the concrete about the same axis. The actual amount of the tension in the steel should equal the compression in the concrete, but there is no principle of mechanics that requires equality of the moments about the neutral axis. The moment in the beam is, therefore, the product of the stress in steel or concrete and the effective depth of the beam, the latter being the depth from the steel up to a point one-sixth of the depth of the concrete beam from the top. This is the method given by the writer. It would standardize design as methods using the coefficient of elasticity cannot do.

Professor Clifford, in commenting on the first point, says, "The concrete at the point of juncture must give, to some extent, and this would distribute the bearing over a considerable length of rod." It is just this local "giving" in reinforced concrete which results in cracks that endanger its safety and spoil its appearance; they also discredit it as a permanent form of construction.

Professor Clifford has informed the writer that the tests on bent rods to which he refers were made on $\frac{3}{4}$-in. rounds, embedded for 12 in. in concrete and bent sharply, the bent portion being 4 in. long. The 12-in. portion was greased. The average maximum load necessary to pull the rods out was 16 000 lb. It seems quite probable that there would be some slipping or crushing of the concrete before a very large part of this load was applied. The load at slipping would be a more useful determination than the ultimate, for the reason that repeated application of such loads will wear out a structure. In this connection three sets of tests described in Bulletin No. 29 of the University of Illinois, are instructive. They were made on beams of the same size, and reinforced with the same percentage of steel. The results were as follows:

Beams 511.1, 511.2, 512.1, 512.2: The bars were bent up at third points. Average breaking load, 18 600 lb. All failed by slipping of the bars.

Beams 513.1, 513.2: The bars were bent up at third points and given a sharp right-angle turn over the supports. Average breaking load, 16 500 lb. The beams failed by cracking alongside the bar toward the end.

Beams 514.2, 514.3: The bars were bent up at third points and had anchoring nuts and washers at the ends over the supports. Average breaking load, 22 800 lb. These failed by tension in the steel.

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Mr. Godfrey.

By these tests it is seen that, in a beam, bars without hooks were stronger in their hold on the concrete by an average of 13% than those with hooks. Each test of the group of straight bars showed that they were stronger than either of those with hooked bars. Bars anchored over the support in the manner recommended in the paper were nearly 40% stronger than hooked bars and 20% stronger than straight bars. These percentages, furthermore, do not represent all the advantages of anchored bars. The method of failure is of greatest significance. A failure by tension in the steel is an ideal failure, because it is easiest to provide against. Failures by slipping of bars, and by cracking and disintegrating of the concrete beam near the support, as exhibited by the other tests, indicate danger, and demand much larger factors of safety.

Professor Clifford, in criticizing the statement that a member which cannot act until failure has started is not a proper element of design, refers to another statement by the writer, namely, "The steel in the tension side of the beam should be considered as taking all the tension." He states that this cannot take place until the concrete has failed in tension at this point. The tension side of a beam will stretch out a measurable amount under load. The stretching out of the beam vertically, alongside of a stirrup, would be exceedingly minute, if no cracks occurred in the beam.

Mr. Mensch says that "the stresses involved are mostly secondary." He compares them to web stresses in a plate girder, which can scarcely be called secondary. Furthermore, those stresses are carefully worked out and abundantly provided for in any good design. To give an example of how a plate girder might be designed: Many plate girders have rivets in the flanges, spaced 6 in. apart near the supports, that is, girders designed with no regard to good practice. These girders, perhaps, need twice as many rivets near the ends, according to good and acceptable practice, which is also rational practice. The girders stand up and perform their office. It is doubtful whether they would fail in these rivet lines in a test to destruction; but a reasonable analysis shows that these rivets are needed, and no good engineer would ignore this rule of design or claim that it should be discarded because the girders do their work anyway. There are many things about structures, as every engineer who has examined many of those erected without engineering supervision can testify, which are bad, but not quite bad enough to be cause for condemnation. Not many years ago the writer ordered reinforcement in a structure designed by one of the best structural engineers in the United States, because the floor-beams had sharp bends in the flange angles. This is not a secondary matter, and sharp bends in reinforcing rods are not a secondary matter. No amount of analysis can show that these rods or flange angles will perform their full duty. Something else must

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Mr. Godfrey. be overstressed, and herein is a violation of the principles of sound engineering.

Mr. Mensch mentions the failure of the Quebec Bridge as an example of the unknown strength of steel compression members, and states that, if the designer of that bridge had known of certain tests made 40 years ago, that accident probably would not have happened. It has never been proven that the designer of that bridge was responsible for the accident or for anything more than a bridge which would have been weak in service. The testimony of the Royal Commission, concerning the chords, is, "We have no evidence to show that they would have actually failed under working conditions had they been axially loaded and not subject to transverse stresses arising from weak end details and loose connections." Diagonal bracing in the big erection gantry would have saved the bridge, for every feature of the wreck shows that the lateral collapse of that gantry caused the failure. Here are some more simple principles of sound engineering which were ignored.

It is when practice runs "ahead of theory" that it needs to be brought up with a sharp turn. It is the general practice to design dams for the horizontal pressure of the water only, ignoring that which works into horizontal seams and below the foundation, and exerts a heavy uplift. Dams also fail occasionally, because of this uplifting force which is proven to exist by theory.

Mr. Mensch says:

"The author is manifestly wrong in stating that the reinforcing rods can only receive their increments of stress when the concrete is in tension. Generally, the contrary happens. In the ordinary adhesion test, the block of concrete is held by the jaws of the machine and the rod is pulled out; the concrete is clearly in compression."

This is not a case of increments at all, as the rod has the full stress given to it by the grips of the testing machine. Furthermore, it is not a beam. Also, Mr. Mensch is not accurate in conveying the writer's meaning. To quote from the paper:

"A reinforcing rod in a concrete beam receives its stress by increments imparted by the grip of the concrete, but these increments can only be imparted where the tendency of the concrete is to stretch."

This has no reference to an adhesion test.

Mr. Mensch's next paragraph does not show a careful perusal of the paper. The writer does not "doubt the advisability of using bent-up bars in reinforced concrete beams." What he does condemn is bending up the bars with a sharp bend and ending them nowhere. When they are curved up, run to the support, and are anchored over the support or run into the next span, they are excellent. In the tests mentioned by Mr. Mensch, the beams which had the rods bent up and "continued over the supports" gave the highest "ultimate values." This is exactly

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the construction which is pointed out as being the most rational, if the rods do not have the sharp bends which Mr. Mensch himself condemns. Mr. Godfrey.

Regarding the tests mentioned by him, in which the rods were fastened to anchor-plates at the end and had "slight increase of strength over straight rods, and certainly made a poorer showing than bent-up bars," the writer asked Mr. Mensch by letter whether these bars were curved up toward the supports. He has not answered the communication, so the writer cannot comment on the tests. It is not necessary to use threaded bars, except in the end beams, as the curved-up bars can be run into the next beam and act as top reinforcement while at the same time receiving full anchorage.

Mr. Mensch's statement regarding the retaining wall reinforced as shown at *a,* Fig. 2, is astounding. He "confesses that he never saw or heard of such poor practices." If he will examine almost any volume of an engineering periodical of recent years, he will have no trouble at all in finding several examples of these identical practices. In the books by Messrs. Reid, Maurer and Turneaure, and Taylor and Thompson, he will find retaining walls illustrated, which are almost identical with Fig. 2 at *a*. Mr. Mensch says that the proposed design of a retaining wall would be difficult and expensive to install. The harp-like reinforcement could be put together on the ground, and raised to place and held with a couple of braces. Compare this with the difficulty, expense and uncertainty of placing and holding in place 20 or 30 separate rods. The Fink truss analogy given by Mr. Mensch is a weak one. If he were making a cantilever bracket to support a slab by tension from the top, the bracket to be tied into a wall, would he use an indiscriminate lot of little vertical and horizontal rods, or would he tie the slab directly into the wall by diagonal ties? This is exactly the case of this retaining wall, the horizontal slab has a load of earth, and the counterfort is a bracket in tension; the vertical wall resists that tension and derives its ability to resist from the horizontal pressure of the earth.

Mr. Mensch states that "it would take up too much time to prove that the counterfort acts really as a beam." The writer proposes to show in a very short time that it is not a beam. A beam is a part of a structure subject to bending strains caused by transverse loading. This will do as a working definition. The concrete of the counterfort shown at *b,* Fig. 2, could be entirely eliminated if the rods were simply made to run straight into the anchoring angle and were connected with little cast skewbacks through slotted holes. There would be absolutely no bending in the rods and no transverse load. Add the concrete to protect the rods; the function of the rods is not changed in the least. M. S. Ketchum, M. Am. Soc. C. E.,* calculates the counterfort as a beam, and the six 1-in. square bars which he uses diagonally do not

* "The Design of Walls, Bins and Elevators."

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Mr. Godfrey.

even run into the front slab. He states that the vertical and horizontal rods are to "take the horizontal and vertical shear."

Mr. Mensch says of rectangular water tanks that they are not held (presumably at the corners) by any such devices, and that there is no doubt that they must carry the stress when filled with water. A water tank,* designed by the writer in 1905, was held by just such devices. In a tank† not held by any such devices, the corner broke, and it is now held by reinforcing devices not shown in the original plans.

Mr. Mensch states that he "does not quite understand the author's reference to shear rods. Possibly he means the longitudinal reinforcement, which it seems is sometimes calculated to carry 10 000 lb. per sq. in. in shear;" and that he "never heard of such a practice." His next paragraph gives the most pointed out-and-out statement regarding shear in shear rods which this voluminous discussion contains. He says that stirrups "are best compared with the dowel pins and bolts of a compound wooden beam." This is the kernel of the whole matter in the design of stirrups, and is just how the ordinary designer considers stirrups, though the books and reports dodge the matter by saying "stress" and attempting no analysis. Put this stirrup in shear at 10 000 lb. per sq. in., and we have a shearing unit only equalled in the cheapest structural work on tight-fitting rivets through steel. In the light of this confession, the force of the writer's comparison, between a U-stirrup, ¾ in. in diameter, and two ¾-in. rivets tightly driven into holes in a steel angle, is made more evident. Bolts in a wooden beam built up of horizontal boards would be tightly drawn up, and the friction would play an important part in taking up the horizontal shear. Dowels without head or nut would be much less efficient; they would be more like the stirrups in a reinforced concrete beam. Furthermore, wood is much stronger in bearing then concrete, and it is tough, so that it would admit of shifting to a firm bearing against the bolt. Separate slabs of concrete with bolts or dowels through them would not make a reliable beam. The bolts or dowels would be good for only a part of the safe shearing strength of the steel, because the bearing on the concrete would be too great for its compressive strength.

Mr. Mensch states that at least 99% of all reinforced structures are calculated with a reduction of 25% of the bending moment in the center. He also says "there may be some engineers who calculate a reduction of 33 per cent." These are broad statements in view of the fact that the report of the Joint Committee recommends a reduction of 33% both in slabs and beams.

Mr. Mensch's remarks regarding the width of beams omit from

* *Engineering News*, September 28th, 1905.

† *The Engineering Record*, June 26th, 1909.

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Mr. Godfrey.

consideration the element of span and the length needed to develop the grip of a rod. There is no need of making a rod any less in diameter than one-two-hundredth of the span. If this rule is observed, the beam with three $\frac{7}{8}$-in. round rods will be of longer span than the one with the six $\frac{5}{8}$-in. rods. The horizontal shear of the two beams will be equal to the total amount of that shear, but the shorter beam will have to develop that shear in a shorter distance, hence the need of a wider beam where the smaller rods are used.

It is not that the writer advocates a wide stem in the T-beam, in order to dispense with the aid of the slab. What he desires to point out is that a full analysis of a T-beam shows that such a width is needed in the stem.

Regarding the elastic theory, Mr. Mensch, in his discussion, shows that he does not understand the writer's meaning in pointing out the objections to the elastic theory applied to arches. The moment of inertia of the abutment will, of course, be many times that of the arch ring; but of what use is this large moment of inertia when the abutment suddenly stops at its foundation? The abutment cannot be anchored for bending into the rock; it is simply a block of concrete resting on a support. The great bending moment at the end of the arch, which is found by the elastic theory (on paper), has merely to overturn this block of concrete, and it is aided very materially in this by the thrust of the arch. The deformation of the abutment, due to deficiency in its moment of inertia, is a theoretical trifle which might very aptly be minutely considered by the elastic arch theorist. He appears to have settled all fears on that score among his votaries. The settlement of the abutment both vertically and horizontally, a thing of tremendously more magnitude and importance, he has totally ignored.

Most soils are more or less compressible. The resultant thrust on an arch abutment is usually in a direction cutting about the edge of the middle third. The effect of this force is to tend to cause more settlement of the abutment at the outer, than at the inner, edge, or, in other words, it would cause the abutment to rotate. In addition to this the same force tends to spread the abutments apart. Both these efforts put an initial bending moment in the arch ring at the springing; a moment not calculated, and impossible to calculate.

Messrs. Taylor and Thompson, in their book, give much space to the elastic theory of the reinforced concrete arch. Little of that space, however, is taken up with the abutment, and the case they give has abutments in solid rock with a slope about normal to the thrust of the arch ring. They recommend that the thrust be made to strike as near the middle of the base of the abutment as possible.

Malverd A. Howe, M. Am. Soc. C. E., in a recent issue of *Engineering News,* shows how to find the stresses and moments in an

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Mr. Godfrey. elastic arch; but he does not say anything about how to take care of the large bending moments which he finds at the springing.

Specialists in arch construction state that when the centering is struck, every arch increases in span by settlement. Is this one fact not enough to make the elastic theory a nullity, for that theory assumes immovable abutments?

Professor Howe made some recent tests on checking up the elastic behavior of arches. He reports* that "a very slight change at the support does seriously affect the values of H and M." The arch tested was of 20-ft. span, and built between two heavy stone walls out of all proportion to the magnitude of the arch, as measured by comparison with an ordinary arch and its abutment. To make the arch fixed ended, a large heavily reinforced head was firmly bolted to the stone wall. Practical fixed endedness could be attained, of course, by means such as these, but the value of such tests is only theoretical.

Mr. Mensch says:

"The elastic theory was fully proved for arches by the remarkable tests, made in 1897 by the Austrian Society of Engineers and Architects, on full-sized arches of 70-ft. span, and the observed deflections and lateral deformations agreed exactly with the figured deformation."

The writer does not know of the tests made in 1897, but reference is often made to some tests reported in 1896. These tests are everywhere quoted as the unanswerable argument for the elastic theory. Let us examine a few features of those tests, and see something of the strength of the claim. In the first place, as to the exact agreement between the calculated and the observed deformations, this exact agreement was retroactive. The average modulus of elasticity, as found by specimen tests of the concrete, did not agree at all with the value which it was necessary to use in the arch calculations in order to make the deflections come out right.

As found by tests on blocks, the average modulus was about 2 700 000; the "practical" value, as determined from analysis of a plain concrete arch, was 1 430 000, a little matter of nearly 100 per cent. Mansfield Merriman, M. Am. Soc. C. E., gives a digest of these famous Austrian tests.† There were no fixed ended arches among them. There was a long plain concrete arch and a long Monier arch. Professor Merriman says, "The beton Monier arch is not discussed theoretically, and, indeed, this would be a difficult task on account of the different materials combined." And these are the tests which the Engineering Profession points to whenever the elastic theory is questioned as to its applicability to reinforced concrete arches. These are the tests that "fully prove" the elastic theory for arches. These are the tests on the basis of which fixed ended reinforced concrete arches

* *Railroad Age Gazette*, March 26th, 1909.

† *Engineering News*, April 9th, 1896.

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Mr. Godfrey.

are confidently designed. Because a plain concrete bow between solid abutments deflected in an elastic curve, reinforced concrete arches between settling abutments are designed with fixed ends. The theorist has departed about as far as possible from his premise in this case. On an exceedingly slender thread he has hung an elaborate and important theory of design, with assumptions which can never be realized outside of the schoolroom or the designer's office. The most serious feature of such theories is not merely the approximate and erroneous results which they give, but the extreme confidence and faith in their certainty which they beget in their users, enabling them to cut down factors of safety with no regard whatever for the enormous factor of ignorance which is an essential accompaniment to the theory itself.

Mr. Mensch says, "The elastic theory enables one to calculate arches much more quickly than any graphical or guess method yet proposed." The method given by the writer* enables one to calculate an arch in about the time it would take to work out a few of the many coefficients necessary in the involved method of the elastic theory. It is not a graphic method, but it is safe and sound, and it does not assume conditions which have absolutely no existence.

Mr. Mensch says that the writer brings up some erratic column tests and seems to have no confidence in reinforced concrete columns. In relation to this matter Sanford E. Thompson, M. Am. Soc. C. E., in a paper recently read before the National Association of Cement Users, takes the same sets of tests referred to in the paper, and attempts to show that longitudinal reinforcement adds much strength to a concrete column. Mr. Thompson goes about it by means of averages. It is not safe to average tests where the differences in individual tests are so great that those of one class overlap those of the other. He includes the writer's "erratic" tests and some others which are "erratic" the other way. It is manifestly impossible for him to prove that longitudinal rods add any strength to a concrete column if, on one pair of columns, identically made as far as practicable, the plain concrete column is stronger than that with longitudinal rods in it, unless the weak column is defective. It is just as manifest that it is shown by this and other tests that the supposedly reinforced concrete column may be weaker.

The averaging of results to show that longitudinal rods add strength, in the case of the tests reported by Mr. Withey, includes a square plain concrete column which naturally would show less compressive strength in concrete than a round column, because of the spalling off at the corners. This weak test on a square column is one of the slender props on which is based the conclusion that longitudinal rods add to the strength of a concrete column; but the weakness of the square concrete column is due to the inherent weakness of brittle

* "Structural Engineering: Concrete."

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Mr. Godfrey. material in compression when there are sharp corners which may spall off.

Mr. Worcester says that several of the writer's indictments hit at practices which were discarded long ago, but from the attitude of their defenders this does not seem to be true. There are benders to make sharp bends in rods, and there are builders who say that they must be bent sharply in order to simplify the work of fitting and measuring them.

There are examples in engineering periodicals and books, too numerous to mention, where no anchorage of any kind is provided for bent-up rods, except what grip they get in the concrete. If they reached beyond their point of usefulness for this grip, it would be all right, but very often they do not.

Mr. Worcester says: "It is not necessary that a stirrup at one point should carry all the vertical tension, as this vertical tension is distributed by the concrete." The writer will concede that the stirrups need not carry all the vertical shear, for, in a properly reinforced beam, the concrete can take part of it. The shear reinforcement, however, should carry all the shear apportioned to it after deducting that part which the concrete is capable of carrying, and it should carry it without putting the concrete in shear again. The stirrups at one point should carry all the vertical tension from the portion of shear assumed to be taken by the stirrups; otherwise the concrete will be compelled to carry more than its share of the shear.

Mr. Worcester states that cracks are just as likely to occur from stress in curved-up and anchored rods as in vertical reinforcement. The fact that the vertical stretching out of a beam from the top to the bottom, under its load, is exceedingly minute, has been mentioned. A curved-up bar, anchored over the support and lying near the bottom of the beam at mid-span, partakes of the elongation of the tension side of the beam and crosses the section of greatest diagonal tension in the most advantageous manner. There is, therefore, a great deal of difference in the way in which these two elements of construction act.

Mr. Worcester prefers the "customary method" of determining the width of beams—so that the maximum horizontal shearing stress will not be excessive—to that suggested by the writer. He gives as a reason for this the fact that rods are bent up out of the bottom of a beam, and that not all of them run to the end. The "customary method" must be described in literature for private circulation. Mention has been made of a method which makes the width of beam sufficient to insert the steel. Considerations of the horizontal shear in a T-beam, and of the capacity of the concrete to grip the steel, are conspicuous by their absence in the analyses of beams. If a reinforcing rod is curved up and anchored over the support, the concrete is relieved of the shear, both horizontal and vertical, incident to the

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Mr. Godfrey.

stress in that rod. If a reinforcing rod is bent up anywhere, and not carried to the support, and not anchored over it, as is customary, the shear is all taken by the concrete; and there is just the same shear in the concrete as though the rods were straight.

For proper grip a straight rod should have a diameter of not more than one two-hundredth of the span. For economy of material, it should not be much smaller in diameter than this. With this balance in a beam, assuming shear equal to bond, the rods should be spaced a distance apart, equal to their perimeters. This is a rational and simple rule, and its use would go a long way toward the adoption of standards.

Mr. Worcester is not logical in his criticism of the writer's method of reinforcing a chimney. It is not necessary to assume that the concrete is not stressed, in the imaginary plain concrete chimney, beyond that which plain concrete could take in tension. The assumption of an imaginary plain concrete chimney and determinations of tensile stresses in the concrete are merely simplified methods of finding the tensile stress. The steel can take just as much tensile stress if its amount is determined in this way as it can if any other method is used. The shifting of the neutral axis, to which Mr. Worcester refers, is another of the fancy assumptions which cannot be realized because of initial and unknown stresses in the concrete and steel.

Mr. Russell states that the writer scarcely touched on top reinforcement in beams. This would come in the class of longitudinal rods in columns, unless the reinforcement were stiff members. Mr. Russell's remarks, to the effect that columns and short deep beams, doubly reinforced, should be designed as framed structures, point to the conclusion that structural beams and columns, protected with concrete, should be used in such cases. If the ruling motive of designers were uniformly to use what is most appropriate in each particular location and not to carry out some system, this is just what would be done in many cases; but some minds are so constructed that they take pleasure in such boasts as this: "There is not a pound of structural steel in that building." A broad-minded engineer will use reinforced concrete where it is most appropriate, and structural steel or cast iron where these are most appropriate, instead of using his clients' funds to carry out some cherished ideas.

Mr. Wright appreciates the writer's idea, for the paper was not intended to criticize something which is "good enough" or which "answers the purpose," but to systematize or standardize reinforced concrete and put it on a basis of rational analysis and common sense, such a basis as structural designing has been or is being placed on, by a careful weeding out of all that is irrational, senseless, and weak.

Mr. Chapman says that the practical engineer has never used such methods of construction as those which the writer condemns. The

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Mr. Godfrey. methods are common enough; whether or not those who use them are practical engineers is beside the question.

As to the ability of the end connection of a stringer carrying flange stress or bending moments, it is not uncommon to see brackets carrying considerable overhanging loads with no better connection. Even wide sidewalks of bridges sometimes have tension connections on rivet heads. While this is not to be commended, it is a demonstration of the ability to take bending which might be relied on, if structural design were on as loose a basis as reinforced concrete.

Mr. Chapman assumes that stirrups are anchored at each end, and Fig. 3 shows a small hook to effect this anchorage. He does not show how vertical stirrups can relieve a beam of the shear between two of these stirrups.

The criticism the writer would make of Figs. 5 and 6, is that there is not enough concrete in the stem of the T to grip the amount of steel used, and the steel must be gripped in that stem, because it does not run to the support or beyond it for anchorage. Steel members in a bridge may be designed in violation of many of the requirements of specifications, such as the maximum spacing of rivets, size of lattice bars, etc.; the bridge will not necessarily fail or show weakness as soon as it is put into service, but it is faulty and weak just the same.

Mr. Chapman says: "The practical engineer does not find * * * that the negative moment is double the positive moment, because he considers the live load either on one span only, or on alternate spans." It is just in such methods that the "practical engineer" is inconsistent. If he is going to consider the beams as continuous, he should find the full continuous beam moment and provide for it. It is just this disposition to take an advantage wherever one can be taken, without giving proper consideration to the disadvantage entailed, which is condemned in the paper. The "practical engineer" will reduce his bending moment in the beam by a large fraction, because of continuity, but he will not reinforce over the supports for full continuity. Reinforcement for full continuity was not recommended, but it was intimated that this is the only consistent method, if advantage is taken of continuity in reducing the principal bending moment.

Mr. Chapman says that an arch should not be used where the abutments are unstable. Unstable is a relative and indefinite word. If he means that abutments for arches should never be on anything but rock, even such a foundation is only quite stable when the abutment has a vertical rock face to take horizontal thrusts. If arches could be built only under such conditions, few of them would be built. Some settlement is to be expected in almost any soil, and because of horizontal thrusts there is also a tendency for arch abutments to rotate. It is this tendency which opens up cracks in spandrels of arches, and makes the assumption of a fixed tangent at the springing line, commonly made by the elastic theorist, absolute foolishness.

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Mr. Godfrey.

Mr. Beyer has developed a novel explanation of the way stirrups act, but it is one which is scarcely likely to meet with more serious consideration than the steel girder to which he refers, which has neither web plate nor diagonals, but only verticals connecting the top and bottom flanges. This style of girder has been considered by American engineers rather as a curiosity, if not a monstrosity. If vertical stirrups acted to reinforce little vertical cantilevers, there would have to be a large number of them, so that each little segment of the beam would be insured reinforcement.

The writer is utterly at a loss to know what Professor Ostrup means by his first few paragraphs. He says that in the first point two designs are mentioned and a third condemned. The second design, whatever it is, he lays at the writer's door in these words: "The author's second design is an invention of his own, which the Profession at large is invited to adopt." In the first point sharp bends in reinforcing rods are condemned and curves recommended. Absolutely nothing is said of "a reinforced concrete beam arranged in the shape of a rod, with separate concrete blocks placed on top of it without being connected."

In reply to Professor Ostrup, it should be stated that the purpose of the paper is not to belittle the importance of the adhesion or grip of concrete on steel, but to point out that the wonderful things this grip is supposed to do, as exhibited by current design, will not stand the test of analysis.

Professor Ostrup has shown a new phase of the stress in shear rods. He says they are in bending between the centers of compressive resultants. We have been told in books and reports that these rods are in stress of some kind, which is measured by the sectional area of the rod. No hint has been given of designing stirrups for bending. If these rods are not in shear, as stated by Professor Ostrup, how can they be in bending in any such fashion as that indicated in Fig. 12?

Professor Ostrup's analysis, by which he attempts to justify stirrups and to show that vertical stirrups are preferable, merely treats of local distribution of stress from short rods into concrete. Apparently, it would work the same if the stirrups merely touched the tension rod. His analysis ignores the vital question of what possible aid the stirrup can be in relieving the concrete between stirrups of the shear of the beam.

The juggling of bending moments in beams is not compensating. The following is a concrete example. Some beams of a span of about 20 ft., were framed into double girders at the columns. The beams were calculated as partly continuous, though they were separated at their ends by about 1½ or 2 ft., the space between the girders. The beams had 1⅛-in. tension rods in the bottom. At the supports a short ¼-in. rod was used near the top of the beam for continuity. Does this

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Mr. Godfrey. need any comment? It was not the work of a novice or of an inexperienced builder.

Professor Ostrup's remarks about the shifting of the neutral axis of a beam and of the pressure line of an arch are based on theory which is grounded in impossible assumptions. The materials dealt with do not justify these assumptions or the hair-splitting theory based thereon. His platitudes about the danger of misplacing reinforcement in an arch are hardly warranted. If the depth and reinforcement of an arch ring are added to, as the inelastic, hinge-end theory would dictate, as against the elastic theory, it will strengthen the arch just as surely as it would strengthen a plate girder to thicken the web and flange angles.

The writer's complaint is not that the theories of reinforced concrete are not fully developed. They are developed too highly, developed out of all comparison with the materials dealt with. It is just because reinforced concrete structures are being built in increasing numbers that it behooves engineers to inject some rationality (not high-strung theory) into their designs, and drop the idea that "whatever is is right."

Mr. Porter has much to say about U-bars. He states that they are useful in holding the tension bars in place and in tying the slab to the stem of a T-beam. These are legitimate functions for little loose rods; but why call them shear rods and make believe that they take the shear of a beam? As to stirrups acting as dowel pins, the writer has already referred to this subject. Answering a query by Mr. Porter, it may be stated that what would counteract the horizontal cleaving force in a beam is one or more rods curved up to the upper part of the beam and anchored at the support or run into the next span. Strangely enough, Mr. Porter commends this very thing, as advocated in the paper. The excellent results shown by the test referred to by him can well be contrasted with some of the writer's tests. This floor was designed for 250 lb. per sq. ft. When that load was placed on it, the deflection was more than 1 in. in a span of 20 ft. No rods were curved up and run over the supports. It was a stirrup job.

Mr. Porter intimates that the correct reinforced concrete column may be on lines of concrete mixed with nails or wires. There is no doubt but that such concrete would be strong in compression for the reason that it is strong in tension, but a column needs some unifying element which is continuous. A reinforced column needs longitudinal rods, but their office is to take tension; they should not be considered as taking compression.

Mr. Goodrich makes this startling remark: "It is a well-known fact that the bottom chords in queen-post trusses are useless, as far as resistance to tension is concerned." The writer cannot think that he means by this that, for example, a purlin made up of a 3 by 2-in. angle and a $\frac{5}{8}$-in. hog-rod would be just as good with the rod omitted. If

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queen-post trusses are useless, some hundreds of thousands of hog-rods in freight cars could be dispensed with. Mr. Godfrey.

Mr. Goodrich misunderstands the reference to the "only rational and only efficient design possible." The statement is that a design which would be adopted, if slabs were suspended on rods, is the only rational and the only efficient design possible. If the counterfort of a retaining wall were a bracket on the upper side of a horizontal slab projecting out from a vertical wall, and all were above ground, the horizontal slab being heavily loaded, it is doubtful whether any engineer would think of using any other scheme than diagonal rods running from slab to wall and anchored into each. This is exactly the condition in this shape of retaining wall, except that it is underground.

Mr. Goodrich says that the writer's reasoning as to the sixth point is almost wholly facetious and that concrete is very strong in pure shear. The joke, however, is on the experimenters who have reported concrete very strong in shear. They have failed to point out that, in every case where great strength in shear is manifested, the concrete is confined laterally or under heavy compression normal to the sheared plane. Stirrups do not confine concrete in a direction normal to the sheared plane, and they do not increase the compression. A large number of stirrups laid in herring-bone fashion would confine the concrete across diagonal planes, but such a design would be wasteful, and the common method of spacing the stirrups would not suggest their office in this capacity.

As to the writer's statements regarding the tests in Bulletin No. 29 of the University of Illinois being misleading, he quotes from that bulletin as follows:

"Until the concrete web has failed in diagonal tension and diagonal cracks have formed there must be little vertical deformation at the plane of the stirrups, so little that not much stress can have developed in the stirrups." * * * "It is evident, then, that until the concrete web fails in diagonal tension little stress is taken by the stirrups." * * * "It seems evident from the tests that the stirrups did not take much stress until after the formation of diagonal cracks." * * * "It seems evident that there is very little elongation in stirrups until the first diagonal crack forms, and hence that up to this point the concrete takes practically all the diagonal tension." * * * "Stirrups do not come into action, at least not to any great extent, until the diagonal crack has formed."

In view of these quotations, the misleading part of the reference to the tests and their conclusion is not so evident.

The practical tests on beams with suspension rods in them, referred to by Mr. Porter, show entirely different results from those mentioned by Mr. Goodrich as being made by Mörsch. Tests on beams of this sort, which are available in America, seem to show excellent results.

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Mr. Godfrey.

Mr. Goodrich is somewhat unjust in attributing failures to designs which are practically in accordance with the suggestions under Point Seven. In Point Seven the juggling of bending moments is condemned—it is condemnation of methods of calculating. Point Seven recommends reinforcing a beam for its simple beam moment. This is the greatest bending it could possibly receive, and it is inconceivable that failure could be due to this suggestion. Point Seven recommends a reasonable reinforcement over the support. This is a matter for the judgment of the designer or a rule in specifications. Failure could scarcely be attributed to this. It is the writer's practice to use reinforcement equal to one-half of the main reinforcement of the beam across the support; it is also his practice to curve up a part of the beam reinforcement and run it into the next span in all beams needing reinforcement for shear; but the paper was not intended to be a treatise on, nor yet a general discussion of, reinforced concrete design.

Mr. Goodrich characterizes the writer's method of calculating reinforced concrete chimneys as crude. It is not any more crude than concrete. The ultra-theoretic methods are just about as appropriate as calculations of the area of a circle to hundredths of a square inch from a paced-off diameter. The same may be said of deflection calculations.

Mr. Goodrich has also appreciated the writer's spirit in presenting this paper. Attention to details of construction has placed structural steel designing on the high plane on which it stands. Reinforced concrete needs the same careful working out of details before it can claim the same recognition. It also needs some simplification of formulas. Witness the intricate column formulas for steelwork which have been buried, and even now some of the complex beam formulas for reinforced concrete have passed away.

Major Sewell, in his discussion of the first point, seems to object solely to the angle of the bent-up portion of the rod. This angle could have been much less, without affecting the essence of the writer's remarks. Of course, the resultant, *b*, would have been less, but this would not create a queen-post at the sharp bend of the bar. Major Sewell says that he "does not remember ever to have seen just the type of construction shown in Fig. 1, either used or recommended." This type of beam might be called a standard. It is almost the insignia of a reinforced concrete expert. A little farther on Major Sewell says that four beams tested at the University of Illinois were about as nearly like Fig. 1 as anything he has ever seen in actual practice. He is the only one who has yet accused the writer of inventing this beam.

If Major Sewell's statement that he has never seen the second point exemplified simply means that he has never seen an example of the bar bent up at the identical angle given in the paper, his criticism has not much weight.

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Major Sewell's comment on the retaining wall begs the question. Specific references to examples have been given in which the rods of a counterfort are not anchored into the slabs that they hold by tension, save by a few inches of embedment; an analysis has also been cited in which the counterfort is considered as a beam, and ties in the great weight of the slab with a few "shear rods," ignoring the anchorage of either horizontal, vertical, or diagonal rods. It is not enough that books state that rods in tension need anchorage. They should not show examples of rods that are in pure tension and state that they are merely thrown in for shear. Transverse rods from the stem to the flange of a T-beam, tie the whole together; they prevent cracking, and thereby allow the shearing strength of the concrete to act. It is not necessary to count the rods in shear. Mr. Godfrey.

Major Sewell's comparison of a stirrup system and a riveted truss is not logical. The verticals and diagonals of a riveted truss have gusset plates which connect symmetrically with the top chord. One line of rivets or a pin in the center line of the top chord could be used as a connection, and this connection would be complete. To distribute rivets above and below the center line of the top chord does not alter the essential fact that the connection of the web members is complete at the center of the top chord. The case of stirrups is quite different. Above the centroid of compression there is nothing but a trifling amount of embedment of the stirrup. If ½-in. stirrups were used in an 18-in. beam, assuming that 30 diameters were enough for anchorage, the centroid of compression would be, say, 3 in. below the top of the beam, the middle point of the stirrup's anchorage would be about 8 in., and the point of full anchorage would be about 16 in. The neutral axis would come somewhere between. These are not unusual proportions. Analogy with a riveted truss fails; even the anchorage above the neutral axis is far from realization.

Major Sewell refers to shallow bridge stringers and the possibility of failure at connections by continuity or deflection. Structural engineers take care of this, not by reinforcement for continuity but by ample provision for the full bending moment in the stringer and by ample depth. Provision for both the full bending moment and the ample depth reduces the possibilities of deflection at the floor-beams.

Major Sewell seems also to have assumed that the paper was a general discussion on reinforced concrete design. The idea in pointing out that a column having longitudinal rods in it may be weaker than a plain concrete column was not to exalt the plain concrete column but to degrade the other. A plain concrete column of any slenderness would manifestly be a gross error. If it can be shown that one having only longitudinal rods may be as bad, or worse, instead of being greatly strengthened by these rods, a large amount of life and property may be saved.

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Mr. Godfrey.

A partial reply to Mr. Thompson's discussion will be found in the writer's response to Mr. Mensch. The fault with Mr. Thompson's conclusions lies in the error of basing them on averages. Average results of one class are of little meaning or value when there is a wide variation between the extremes. In the tests of both the concrete-steel and the plain concrete which Mr. Thompson averages there are wide variations. In the tests made at the University of Illinois there is a difference of almost 100% between the minimum and maximum results in both concrete-steel and plain concrete columns.

Average results, for a comparison between two classes, can mean little when there is a large overlap in the individual results, unless there is a large number of tests. In the seventeen tests made at the University of Illinois, which Mr. Thompson averages, the overlap is so great that the maximum of the plain columns is nearly 50% greater than the minimum of the concrete-steel columns.

If the two lowest tests in plain concrete and the two highest in concrete-steel had not been made, the average would be in favor of the plain concrete by nearly as much as Mr. Thompson's average now favors the concrete-steel columns. Further, if these four tests be eliminated, only three of the concrete-steel columns are higher than the plain concrete. So much for the value of averages and the conclusions drawn therefrom.

It is idle to draw any conclusions from such juggling of figures, except that the addition of longitudinal steel rods is altogether problematical. It may lessen the compressive strength of a concrete column. Slender rods in such a column cannot be said to reinforce it, for the reason that careful tests have been recorded in which columns of concrete-steel were weaker than those of plain concrete.

In the averages of the Minneapolis tests Mr. Thompson has compared the results on two plain concrete columns with the average of tests on an indiscriminate lot of hooped and banded columns. This method of boosting the average shows anything but "critical examination" on his part.

Mr. Thompson, on the subject of Mr. Withey's tests, compares plain concrete of square cross-section with concrete-steel of octagonal section. As stated before, this is not a proper comparison. In a fragile material like concrete the corners spall off under a compressive load, and the square section will not show up as well as an octagonal or round one.

Mr. Thompson's contention, regarding the Minneapolis tests, that the concrete outside of the hoops should not be considered, is ridiculous. If longitudinal rods reinforce a concrete column, why is it necessary to imagine that a large part of the concrete must be assumed to be non-existent in order to make this reinforcement manifest? An imaginary core could be assumed in a plain concrete column and any desired results could be obtained. Furthermore, a properly hooped

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Mr. Godfrey

column does not enter into this discussion, as the proposition is that slender longitudinal rods do not reinforce a concrete column; if hoops are recognized, the column does not come under this proposition.

Further, the proposition in the writer's fifteenth point does not say that the steel takes no part of the compression of a column. Mr. Thompson's laborious explanation of the fact that the steel receives a share of the load is needless. There is no doubt that the steel receives a share of the load—in fact, too great a share. This is the secret of the weakness of a concrete column containing slender rods. The concrete shrinks, the steel is put under initial compression, the load comes more heavily on the steel rods than on the concrete, and thus produces a most absurd element of construction—a column of slender steel rods held laterally by a weak material—concrete. This is the secret of nearly all the great wrecks in reinforced concrete: A building in Philadelphia, a reservoir in Madrid, a factory in Rochester, a hotel in California. All these had columns with longitudinal rods; all were extensive failures—probably the worst on record; not one of them could possibly have failed as it did if the columns had been strong and tough. Why use a microscope and search through carefully arranged averages of tests on nursery columns, with exact central loading, to find some advantage in columns of this class, when actual experience is publishing in bold type the tremendously important fact that these columns are utterly untrustworthy?

It is refreshing to note that not one of the writer's critics attempts to defend the quoted ultimate strength of a reinforced concrete column. Even Mr. Thompson acknowledges that it is not right. All of which, in view of the high authority with whom it originated, and the wide use it has been put to by the use of the scissors, would indicate that at last there is some sign of movement toward sound engineering in reinforced concrete.

In conclusion it might be pointed out that this discussion has brought out strong commendation for each of the sixteen indictments. It has also brought out vigorous defense of each of them. This fact alone would seem to justify its title. A paper in a similar strain, made up of indictments against common practices in structural steel design, published in *Engineering News* some years ago, did not bring out a single response. While practice in structural steel may often be faulty, methods of analysis are well understood, and are accepted with little question.

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AMERICAN SOCIETY OF CIVIL ENGINEERS
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TRANSACTIONS

Paper No. 1170

THE WATER SUPPLY OF THE EL PASO AND SOUTHWESTERN RAILWAY FROM CARRIZOZO TO SANTA ROSA, N. MEX.*

By J. L. Campbell, M. Am. Soc. C. E.

With Discussion by Messrs. G. E. P. Smith, Kenneth Allen, and J. L. Campbell.

Location.—The El Paso and Southwestern Railway traverses the arid country west of the 100th Meridian in New Mexico, Texas, and Arizona, as shown on the map, Fig. 1. The water supply herein described serves that division of this road lying between Carrizozo and Santa Rosa, a distance of 128 miles.

Rainfall.—The average annual precipitation is 9.84 in. The year 1909 was exceptionally dry, with a rainfall of less than 5 in.

Original Water Supply.—East and west of El Paso, for distances of 270 miles in each direction, the railway crosses no streams, and the supply was obtained from wells ranging from 100 to 1 100 ft. in depth. On the division served by the new supply, this well-water is of very bad quality, as shown in Table 1.

After the most thorough practicable treatment, these waters were still so bad that they caused violent foaming, low steam pressure, hard scaling, rapid destruction of boiler tubes, high coal and water consumption, extraordinary engine failures and repairs, small engine

* Presented at the meeting of May 4th, 1910.

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mileage, low train tonnage, excessive overtime, and a demoralized train service.

TABLE 1.

Station.	Incrusting solids, in grains per gallon.	Non-incrusting solids, in grains per gallon.
Carrizozo	81	7
Ancho	14	14
Gallinas	91	8
Varney	180	14
Duran	127	55
Tony	115	11
Pastura	141	6
Pintado	81	9
Santa Rosa	140	29

New Water Supply.—The writer was directed to find, if possible, a supply of good water, and his efforts proved successful. The pure water now in use has eliminated the adverse conditions before mentioned; has improved the *esprit de corps* of the train service; and, in a short time, the reduction in operating expenses will liquidate the first cost of the new supply.

This supply is taken from the South Fork of Bonito Creek, which flows down the eastern slope of White Mountain. The latter is 12 000 ft. high, and is 16 miles south of Carrizozo (Fig. 1). The watershed is a granite and porphyry formation, heavily timbered, and the stream is fed by snow and rain. This combination yields an excellent water, carrying on an average 6.05 grains of incrusting and 0.95 grains of non-incrusting solids per gallon. The North Fork of the creek carries 16.60 and 2.40 grains, respectively. Below the junction of these forks, the water contains 10.48 grains of incrusting and 1.57 grains of non-incrusting solids per gallon; and a branch pipe line takes water from the creek during intervals in dry years when the daily flow of the South Fork is less than the consumption.

The Water Plant.—The water is taken to and along the railway in pipe lines. The system includes 116 miles of wood pipe, 19 miles of iron pipe, one 422 000 000-gal. storage reservoir, four 2 500 000-gal. service reservoirs, two pumping plants in duplicate, and accessories of valves, stand-pipes, etc.

From a small concrete dam across the creek at an elevation of 7 728 ft., the pipe line drops down the narrow valley eastward, 5½ miles, to an elevation of 6 980 ft., where it turns abruptly north, rising in

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1 mile to a table-land, 7 215 ft. above sea level, across which it continues northward 5 miles to the storage reservoir, which is on the north edge of this elevated country. Hereafter, this reservoir will be called the Nogal Reservoir, from the old mining village of Nogal lying 1½ miles to the north and 600 ft. below it. From this reservoir, the line drops abruptly to the Carrizozo plain, and crosses the latter northward

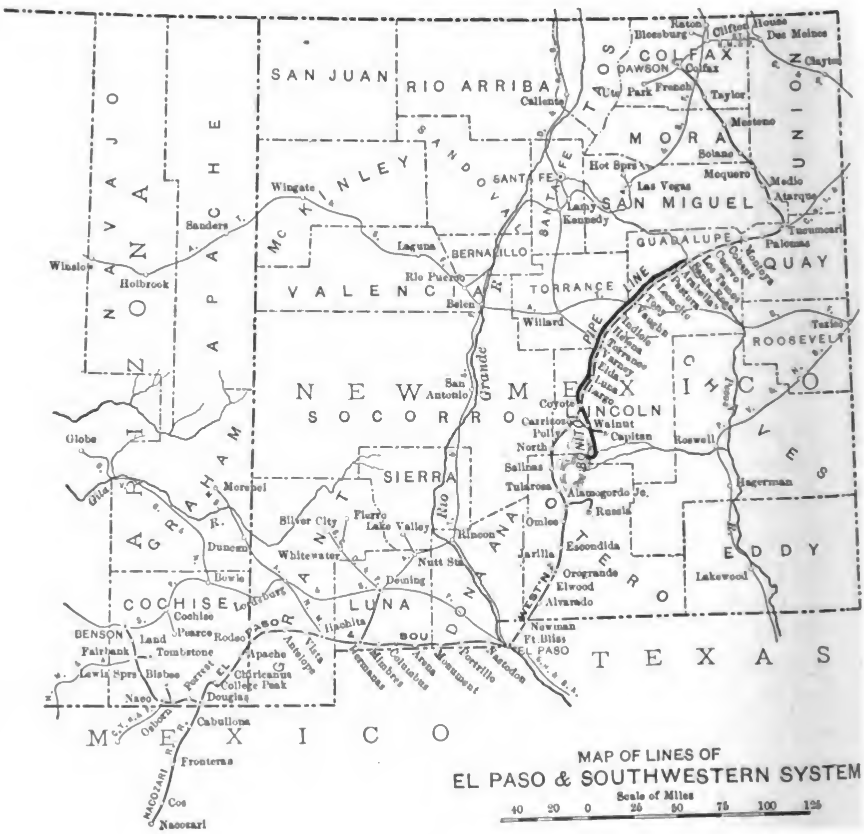

Fig. 1.

to Coyote, at Mile 156, on the railway, at an elevation of 5 810 ft., passing, on the way, 6 miles east of Carrizozo, to which a branch pipe runs, Carrizozo being 5 430 ft. above sea level. There is a 2 500 000-gal. reservoir at Coyote, and a similar one at Carrizozo.

This describes the gravity section of the line which brings the water from the mountain stream to the railway. From Nogal Reservoir to

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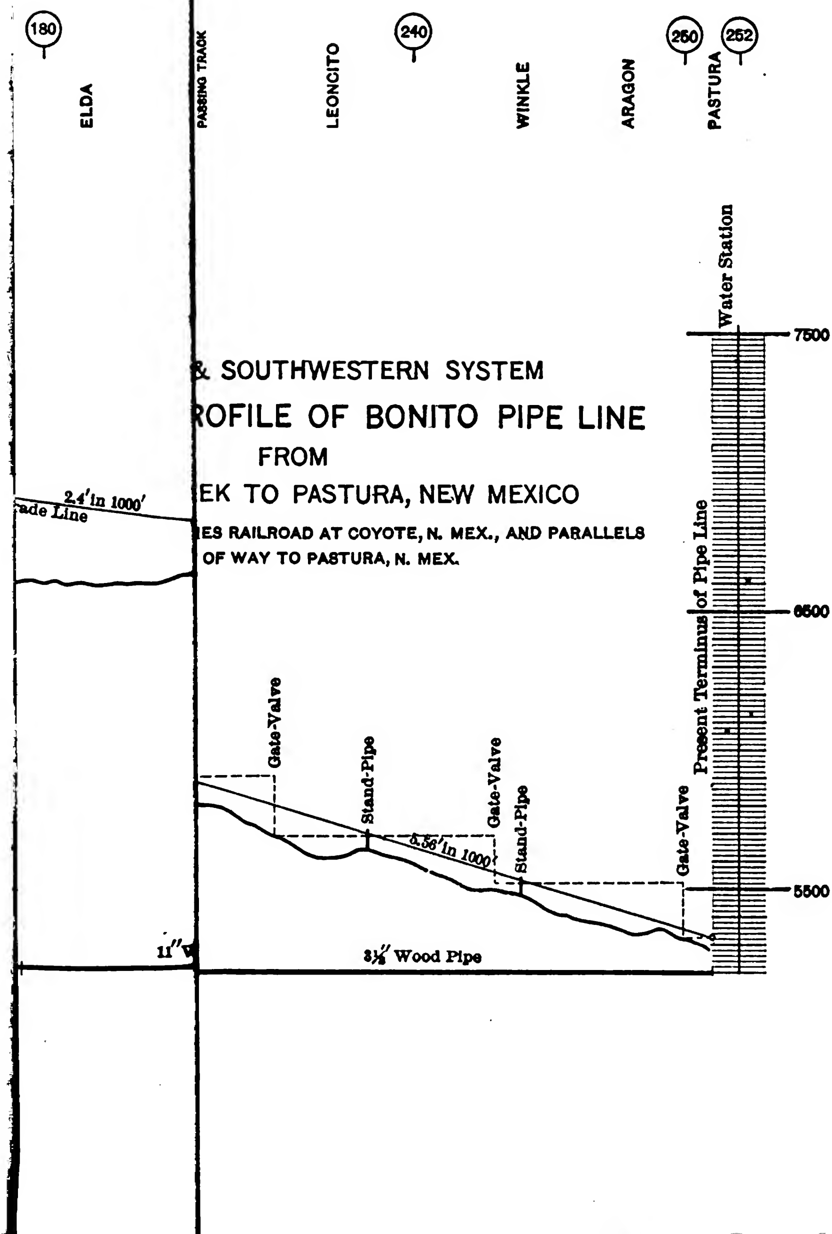
PLATE V.
TRANS. AM. SOC. CIV. ENGRS.
VOL. LXX, No. 1170.
CAMPBELL ON
RAILROAD WATER SUPPLY.



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the latter, the capacity of the pipe is equal to the future daily requirements; from the source of supply to the reservoir, the pipe has twice as great a capacity, thereby storing surplus water. This section is 32 miles long, with a 6-mile branch line.

The second, or pumping section, extends eastward along the railway, rising from an elevation of 5 810 ft. at Coyote to 6 750 ft. on the Corona summit, which is the water-shed line between the Rio Grande on the west and the Rio Pecos on the east. At Coyote a pumping station lifts the water to Luna Reservoir and the pumps at Mile 171, and the latter lift it to the reservoir on Corona summit at Mile 192½. This section is 36½ miles long.

The third, or gravity section, extends from the reservoir on the Corona summit to the Rio Pecos at Mile 272, dropping from an elevation of 6 750 to 4 570 ft. in 80 miles. The pipe line extends to Pastura, 58½ miles from Corona, as shown on Plate V.

Where the pipe line passes a water tank on the railway, a 4-in. branch pipe is carried to the bottom of the tank and up to the top, where it is capped by an automatic valve. A gate-valve is placed in the branch pipe at its junction with the pipe line.

There are regulating, relief, check, blow-off, and air-valves, air-chambers, and open stand-pipes on the line, too numerous to mention in detail. They are designed to keep the wood pipe full, regulate flow, prevent accumulation of pressure and water-hammer, and remove sediment.

Water Pipe.—A study of the profile developed a system of hydraulic grades, pipe diameters, and open stand-pipes limiting the pressure to 130 lb. per sq. in., except on 19 miles of the pump main between Coyote and Corona where the estimated maximum pressure is 310 lb.

Investigation justified the assumption that wood pipe under a pressure of 130 lb. would give satisfactory service for 25 years, on which basis it would be less expensive than cast iron, and therefore it was used. Cast iron was considered preferable to steel for pressures not exceeding 310 lb. on account of its greater durability.

Wood Pipe.—Machine-made, spirally-wound, wood-stave pipe, made in sections from 8 to 12 ft. long, with the exterior surface covered with a heavy coat of asphalt, was selected in preference to unprotected, continuous, stave pipe. The diameters were not so great as to require the latter.

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The first 40 miles of wood pipe was furnished by the Wykoff Wood Pipe Company, of Elmira, N. Y., and the Michigan Pipe Company, of Bay City, Mich., delivered the remaining 76 miles.

The pipe is wound with flat steel bands of from 14 to 18 gauge and from 1 to 2 in. wide. The machine winds at any desired pitch and tension. At each end the spiral wind is doubled two turns, the second lying over the first and developing a frictional resistance similar to that of a double hitch of a rope around a post. The ends of the band are held by screw nails or a forged clip, the latter being the better. It has two or three spikes on the under side which seat into the stave, and two side lugs on top which turn down over the band. The latter passes twice over the seat on the clip, the first turn holding the clip to the stave, while the second turn is held by the lugs which are hammered down over it. The end of the band is then turned back over the clip and held down by a staple.

The staves are double-tongued and grooved and from 1⅝ to 2 in. thick. The smaller thickness is sufficient. The exterior face of the staves should be turned concentric with the axis of the pipe and form a circle, so that the band will have perfect contact with the wood.

The joints are formed by turning a chamber in one end of the pipe and a tenon on the other, or both ends are turned to a true exterior circle and driven into a wood or steel sleeve. The chamber and tenon were used in this work.

Finally, each piece of pipe is covered with as much hot asphalt as it will carry.

Steel Bands.—The specifications required bands of mild steel, of 60 000 lb. strength, with an elastic limit half as great. The winding was spaced to limit the tension to 15 000 lb. per sq. in. If severe water-hammer is present, the ordinary working stress should be materially less than the latter, otherwise the spiral bands will stretch enough to permit the water to spurt out between the staves. This was determined to be true on 4 500 ft. of 12-in. pipe connecting the Carrizozo Reservoir with a water column at the roundhouse there. In pumping tests at the mills, attempts were made, at various times, to burst the pipe, but they never succeeded. Before the elastic limit was exceeded, the water was running out between the staves as fast as the pump forced it in. On the following day, pipe thus tested would carry the pressure for which it was designed without leaking. Except for defects in the band, pipe

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of this kind will not burst in the service for which it is properly designed. This is true, without exception, of the 100 000 pieces of pipe in this service.

There has been some trouble with a number of the riveted splices on the banding. Such a splice occurs for every spool of banding used. In every case where one of these splices has pulled apart, the break was the result of defective riveting, permitting the rivets to pull out. In no case has a rivet been found sheared off, and even one good rivet appears to be sufficient to prevent rupture. The explanation is found in the high frictional resistance between the band and the pipe, which distributes the weakness of a bad splice over several adjacent turns of the band around the pipe. The band loosens a few turns only on either side of a parted splice, generally not more than three. In no case has any pipe been removed from the trench, repairs being made without interruption to the flow of water.

It is desirable to substitute welding for the riveting of these splices. The trouble is not present with the round band, the wrapped splice of the latter giving practically 100% efficiency.

The flat band was chosen for this work because it is the more effectively buried in and protected by the asphalt, and will not crush the soft wood staves under high pressure. The longevity of either the flat or the round steel band is dependent primarily on effective protection against contact with corrosive elements. Wrought iron should be used for this kind of service, and, for the same reason, for many other purposes. Engineers and consumers should join in some comprehensive and effective plan to bring back the old-time production of high-grade wrought iron.

Wood Staves.—The staves of this pipe are of Michigan and Canadian white pine. This pine cannot now be had of clear stuff or in long lengths in large quantities; otherwise, it is unexcelled. Douglas fir and yellow pine, coarser and harder woods, have the advantages of clear lumber and long length. Cypress is not as plentiful, and redwood is costly. The mill tests did not determine definitely the minimum degree of seasoning necessary, and press of time compelled the acceptance of some rather green lumber. Service tests do not show that there is any abnormal leakage from pipe made of such lumber, and it could not now be distinguished in the trench by such tests. Undoubtedly, however, thorough air seasoning should be required.

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Bored Pipe.—Owing to its small size, a part of the 3½-in. pipe was bored from the log. This was a mistake, for bored pipe has a rough interior and a reduced capacity. The inspection and culling are difficult and unsatisfactory, and imperfections readily apparent in a stave frequently escape detection in bored pipe.

Pipe Joints.—The chamber and tenon of this pipe is an all-wood joint, 4 in. deep. An iron sleeve makes a better and stronger joint. It compensates for any lack of initial tension in the banding over the chamber of the wood joint, and secures full advantage of the swelling of the wood. Cast iron is better than steel; it is more rigid, and its granulated surface breaks up the smoothness of the wood surface swelling against it. One objection to the cast-iron sleeve is that of cost, but it adds 4 in. to the effective length of every section of pipe, as compared with the wood joints. On the Pacific Coast, a banded wood-stave sleeve is used with success.

Coating.—To preserve the banding from corrosion and the wood from exterior decay, the pipe is thoroughly enveloped in refined asphalt having a flow-point adjusted to the prevailing temperature during shipment and laying. One grade can be used through a considerable range of temperature. This coating endured a 2 000-mile shipment successfully. Each piece was carefully inspected along the trench, and any break in the coating was thoroughly painted with hot asphalt. Enough of the latter came in barrels, with the pipe, from the factory.

The first 37 miles of this pipe has been in service for two years. Recent inspections show the coating to be in excellent condition and the steel underneath to be bright and clean. In some cases, where the initial pressure and leaking between the staves of the dry pipe were great, the escaping air and water lifted the coating into bubbles. At some points where this lifting was great enough to rupture the asphalt, and the soil is heavily charged with alkali, some corrosion has begun.

The integrity and impermeability of this asphalt coat are quite as vital as constant saturation. This coating protects the entire pipe from exterior contact with destructive agencies. With such effective exterior protection, a constantly full pipe is not so imperative. In the exterior protection of the wood, this coated pipe has quite an advantage over continuous stave pipe.

Each piece of pipe goes directly from the winder to the asphalt rolls, then to an adjacent saw-dust table, then back to the rolls, then

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to the table again, and then to the dry finishing rolls at the opposite end of the table. The coating thus consists of two layers of asphalt and two of saw-dust. When the pipe leaves the finishing rolls, the coat is hard and smooth and about $\frac{3}{16}$ in. thick. This describes the coating as done at Bay City, Mich.

At Elmira, N. Y., one application of asphalt and saw-dust only, without a finishing dry roll, completed the work; but the band was run through a bath of hot asphalt as it was wound, thus coating its underside also. This initial treatment of the band on the Wykoff pipe is necessary because the exterior of the stave is neither planed nor turned to a circle. The exterior of the pipe forms a polygon, and the band is in perfect contact only at the angles. The theory in regard to the Michigan pipe is that the perfect contact of the band and the wood on the true exterior circle excludes air from the under surface of the metal, and prevents corrosion. Experience appears to justify this theory.

Cast-Iron Pipe.—Beginning at the first pumping plant at Coyote, at Mile 156, and running up to Mile 166, and again commencing at the Luna pumps, at Mile 171, and extending up to Mile 179, the minimum pressure on those portions of the pump main is more than the 130 lb. per sq. in. allowed for wood pipe, and the final estimated maximum pressures run up to 310 lb.

The selection of iron pipe for these pressures was, first, as between steel and cast-iron; and, second, as between the lead joint of the standard bell and spigot pipe and the machined iron joint of the universal joint pipe. Again, the choice was as between lead and leadite for the bell and spigot pipe.

Cast iron was selected because of the certainty of its long life, and the bell and spigot pipe was selected on the basis of comparative costs for pipe laid. The standard lead joint was chosen on the result of tests. This cast-iron pumping main has a diameter of 12 in. throughout.

Pipe Weights.—Makers of standard bell and spigot pipe urged the usual heavy weights selected for municipal service and heavy water-hammer. Three pressures, *viz.,* 217, 260, and 304 lb., were used for the division of pipe weights, on which the standard pipe-makers specified shell thicknesses of 0.82, 0.89, and 0.97 in. Eliminating water-hammer and adopting a working stress of 2 400 lb., the thick-

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nesses are reduced to 0.54, 0.65, and 0.76 in. To make the latter conform to the specifications of the New England Water-Works Association, the pipe was cast to 0.57, 0.65, and 0.77 in. The reduction in cost amounts to $52 811.

By the provision of air-cushions, hereafter described, the writer's anticipation of no water-hammer on the pumping main has been fully realized.

The pipe was manufactured and inspected under the above-mentioned specifications.

Pipe Joints.—There was a question about the reliability of the lead joint at 300 lb. The writer had a section of 12-in. pipe, with standard joints containing 22 lb. of lead, laid and tested to 500 lb. without sign of failure or leakage. The joints were caulked down $\frac{3}{16}$ in. below the face of the bell. Of 8 700 joints thus made in the field, not one has blown out or failed. A few weeped slightly on top, and they were made permanently tight by additional caulking. The present maximum pressure is 278 lb. These joints are the standard joints specified by the New England Water-Works Association. It should be borne in mind that there is no water-hammer on this line. In 8 700 joints, 198 000 lb. of lead and 3 200 lb. of oakum were used, or 22.76 and 0.37 lb. per joint.

Leadite was tested in competition with lead, but it leaked at 100 lb. and failed under a sustained pressure of 300 lb. It is a friable material, and cannot be caulked successfully. Its principal ingredient appears to be sulphur. The failure was by slow creeping out of the joints. It is melted and poured, but not caulked. It has attractive features for low pressures and for lines not subject to movement or heavy jarring.

Air-Cushions.—To prevent water-hammer on the pumping main, all pumps are provided with large air-chambers. In addition, and as the special feature for absorbing the shock of pumping under high pressure through a pipe 21 miles long, a large air-chamber in the form of a closed steel cylinder, 5 ft. in diameter and 15 ft. long, is mounted on the pumping main outside of the pump-house. This cylinder is set on its side, in concrete collars, directly over the pipe beneath, to which it is connected by a 12-in. tee, in which a 12-in. gate-valve is set. The cylinder is provided with a glass gauge, cocks, etc. It was designed for a working pressure of 300 lb., and, at each pumping plant, it has proved to be entirely air- and water-tight. As indicated by sensi-

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tive gauges on the pump main, just beyond these large air-chambers, the latter absorb all the water-hammer which gets beyond the air-chamber on the pumps.

Air-Pumps.—Each pumping plant is provided with four automatic air-charging devices, connecting to all air-chambers of the pumps and to the air-chamber on the pumping main. They are of the Nordberg type, and have proved very efficient. They are operated only a part of the time; otherwise, they accumulate too much air in the chambers.

Air-Valves.—On the entire line there are 144 automatic air-valves made by the United States Metal Manufacturing Company, of Berwick, Pa. They are working satisfactorily.

Gate-Valves.—In addition to the customary gate- and check-valves at the reservoirs and pumping stations, gate-valves are located at necessary points and elevations in the line to control the flow of water and keep the pipe full, even to the extent of closing all such valves tight and holding the line full without flow. This is for the purpose of delivering through a full pipe any desired quantity of water less than that required to keep the open pipe full. This, of course, is on account of the wood pipe. As the differences of elevations are very great on the gravity sections of the line, and as any one valve might inadvertently become closed tight when other valves above would be open, the bursting of the pipe under such conditions is prevented either by a pressure relief valve attached to and immediately above the gate-valve, or by an open stand-pipe erected on some suitable elevation between the valves. This is more clearly shown on the profile, Plate V, of the ground line and the hydraulic grades of the pipe line. An inspection of this profile will show that these controlling valves are located so that, when closed, the pressure against them does not rise above the maximum pressure on the section above, due to the hydraulic grade of the line when carrying its full capacity.

Safety Valves.—To prevent rupture of the pipe or injury to the pumps, in case the pumping mains should become obstructed, a 6-in. pop safety valve is mounted on the main just beyond the large air-chamber already described. These valves are set to release at the maximum working pressure of the pumps when the regular quantity of water is being pumped, and they are piped to the adjacent reservoir, so that there is no loss from them.

Check-Valves.—Check-valves are placed in the pumping main to

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prevent the backward flow of water. There is one near the pumps, and one at the upper end and outside of the reservoir into which the main discharges.

Blow-Off Valves.—These valves are located in all material valleys or depressions.

Stand-Pipes.—Between the gate-valves, at certain points where the maximum hydraulic grade is not more than 60 ft. above the surface of the ground, open stand-pipes are erected. If the grade line is too high, relief-valves are used, as stated. Also at two points, where a steep grade ends near the ground surface and is followed by a flatter grade, stand-pipes are erected.

These stand-pipes are of 6-in. iron pipe standing in a special casting in the pipe line and enclosed in a concrete base. They are, of course, open at the top, and vary in height from 15 to 60 ft., depending on the elevation of the hydraulic grade. They have given some checks on the position of this grade during the velocity measurements hereinafter described. Their locations are shown on the profile, Plate V.

Nogal Reservoir.—Nogal Reservoir is the storage unit of the system, and is on the north edge of a table-land, 1 700 ft. above the railway, on the Carrizozo plain, 15 miles away. It is 11½ miles from the head of the pipe on Bonito Creek.

This reservoir is a natural basin or bowl, ½ mile in diameter across the top, ¼ mile on the bottom, and 36 ft. deep. A level line, 1 500 ft. long, drawn from its bottom, comes out to grade on the north declivity of the table-land. On this level line an open cut was made and the outlet pipe laid. The cut was then closed by a dam.

The supply pipe from Bonito Creek delivers water into the basin over the top of its southern rim, the water, as it leaves the pipe, flowing over a standard weir, without end contractions, into a stone gutter. A by-pass pipe, with suitable valves, passes around the western side of the basin and connects to the outlet pipe.

This comparatively small amount of work equipped a very good natural reservoir with a capacity of 422 000 000 gal., which can be increased to 1 000 000 000 gal. by embankments across low places in the rim.

Service Reservoirs.—At Coyote, an artificial service reservoir, 100 by 200 ft. on the bottom, with slopes of 1½ on 1 and a total depth of 15 ft., serves as an equalizer of the flow to and away from the

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pumps at that point. The pump-house is built alongside this reservoir. The delivery pipe from the Nogal Reservoir runs directly to the pumps, but has a tee-branch, 50 ft. long, into the Coyote Reservoir. This branch passes through a valve chamber between the pump-house and the reservoir. In this chamber there are controlling valves and an automatic overflow. This overflow is provided against the contingency of a full reservoir and idle pumps. If the pipe line is delivering water faster than the pumps discharge it, the surplus goes into the reservoir. This arrangement is self-acting and controlling. There is a similar arrangement at the Luna pumping plant, also at the Carrizozo service reservoir, and at the regulating reservoir on the Corona summit.

Each of the four service reservoirs is of the same size, and lined with 4 in. of 1:2:4 concrete. At Luna and Corona the concrete is reinforced with $\frac{3}{8}$-in. round rods spaced 12 in. from center to center, both ways. This reinforcement should have been used in all the work.

Pumping Plants.—The pumps at Coyote and Luna are Nordberg duplex, cross-compound, condensing, crank-and-fly-wheel machines, with 6-in. plungers, traveling 600 ft. per min. at full normal speed, and designed to work against 300 lb. per sq. in. They have a guaranteed efficiency of 135 000 000 ft-lb. per 1 000 lb. of steam at 150 lb. and superheated 75 degrees.

The boilers are 125-h.p., Sterling, water-tube, with Foster superheaters, and 33-in. stacks, 100 ft. high.

Each plant is in complete duplicate pump and boiler units, only one set working at a time.

The pump building is a substantial concrete, brick, and steel structure, 50 by 80 ft. in plan, with a fire-wall, with two steel doors dividing the floor space into an engine-room 50 by 50 ft., and a boiler-room 50 by 30 ft. A concrete coal-bin adjoins the exterior boiler-room door. Coal is delivered directly from the car to the bin.

The plant is lighted by a small, but very complete, engine and dynamo on one base and run by steam from the Sterling boilers.

The two plants are exactly alike throughout.

Reservoir Leakage.—The Nogal Reservoir basin is covered with from 2 to 5 ft. of good clay, except where it is punctured by a dike, or washed down to the underlying sandstone by a few gullies. These punctures or washes were covered or filled with clay from 1 to 4 ft. deep. During the first season the leakage, above the 6-ft. contour, was at the rate of 2 in. per day.

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As the water fell, due to leakage, evaporation, and use, a herd of from 300 to 400 cattle were worked around the shore line. This reduced the leakage to $\frac{3}{8}$ in. below 8 ft., and to nothing below 6 ft., above the outlet. As the flow line rises higher each season, the puddling will be continued to the top. The leakage at 12 ft. above the outlet, or 17 ft. above the bottom, is still approximately 1 in. per day. The total puddling, to date, covering two seasons, is equivalent to 11 150 days' work of one cow, and covers an area of 1 500 000 sq. ft.

The clay packed densely, the final hoof marks being not more than $\frac{1}{4}$ in. deep and remaining distinct under the water around the shore line for one year. Apparently, the reservoir will finally become water-tight at all elevations.

The soil in which the four service reservoirs on the railway are built proved to be about the worst for such work. In its natural state on the prairie, after the excavation for the reservoir was completed, it filtered water at the rate of 3 ft. per day. Tamping and puddling still left a filtration of 12 in. per day, with a tendency to increase. Enough water filtered through the concrete to produce settlement and cracks. Finally, the concrete was water-proofed with two coats of soap, two of alum, and one of asphalt. This has made all the reservoirs water-tight. Elaterite, an asphalt paint made by the Elaterite Paint and Manufacturing Company, of Des Moines, Iowa, was used successfully on the Luna Reservoir. This paint is applied cold, and preliminary tests showed it to be quite efficient.

The analysis of the soil is as follows:

Loss on ignition	3.35
Silica	56.36
Oxide of iron	2.93
Oxide of aluminum	8.97
Calcium oxide	15.95
Magnesium oxide	0.98
Oxides of sodium and potassium	0.47
Carbonic acid	11.35
Sulphuric acid	0.11
Chlorine	0.04
Manganese	Traces
	100.51

Insoluble matter, 64.50 per cent.

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Pipe-Line Leakage.—There is no measurable leakage from the iron pipe. By thorough inspection and measurement at the end of two years, leakage on the wood pipe, between Coyote and Bonito Creek, from the 11- and 12-in. pipe, was found to be as follows:

On 8.6 miles,	11-in. pipe,	146 600 gal. per day	=	17 046 gal. per mile.
" 4 "	12 " "	14 829 " " "	=	3 702 " " "

The 7½-in. pipe on this section appears to be leaking less than the 12-in. pipe. Inspection and measurement of it are to be made in a short time.

There is no material leakage from the 10- and 16-in. pipe between Bonito Creek and Nogal Reservoir, as determined by velocity and volumetric measurements hereafter described. The greatest probable error in the velocity measurements would not exceed ½ per cent. If such error existed, and was all charged to leakage, it would amount to but 17 204 gal. per day, or 1 582 gal. per mile, out of a daily delivery of 3 784 000 gal.; but the measured discharge of the pipe, as determined by the velocity, was 5.84 sec-ft., while the mean maximum volume of this water over the weir at the end of the pipe is recorded by the weir as 5.88 sec-ft.

From Coyote, east along the railway, the wood pipe is remarkably tight. The rate of leakage from it, as determined by 600 observations uniformly distributed, was as follows:

11-in. pipe	=	120 gal. per mile per day.
8½- and 7½-in. pipe	=	268 " " " " "

The maximum rate on 1 mile was 1 613 gal. The minimum found was zero.

The observations were made by uncovering a joint and measuring the leakage therefrom for 10 min. A graduated glass measuring to drams was used. The rate of leakage varied from 5 drops to 45 oz. in 10 min. Of the joints uncovered 57% was found to be leaking. It is rather remarkable that, in the large leakage of the 11- and 12-in. pipe between Coyote and Bonito, only one out of every eight joints was leaking. This indicates a physical defect in such joints. The largest leak found on one joint was at the rate of 17 280 gal. per day. Leakage between or through the staves is not measurable, as it is not fast enough to come away in drops unless there is some imperfection in the wood.

The insignificant leakage of 120 gal., stated above, is from the

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11-in. pipe in the pumping main between Coyote and Corona. The present maximum working pressure on it is 100 lb. per sq. in. All the figures given above include visible and invisible leakage, the latter being such as does not appear on the surface. The visible leakage is but a small part of the total.

Stopping the Leaks.—Generally, any ordinary leak is readily stopped by pine wedges. Sometimes a loose joint requires individual bands bolted around it. Bran or saw-dust is effective in stopping the small leaks which cannot be reached by the wedges. The good effect of the latter is likely to be destroyed by a rapid emptying of the pipe. If the water is drawn out faster than the air can enter through the air-valves, heavy vacuums are formed down long slopes, and the air forces its way in through the joints and between the staves. The result is that the pipe will frequently leak badly for some time after it is refilled, although it may have been tight previously.

A full pipe and a steady pressure are highly desirable. This doubtless accounts to some extent for the extreme tightness of the wood pipe in the pumping main.

Grade Lines.—The hydraulic grade lines, shown on Plate V, were laid as best fitting the controlling elevations. The various diameters of pipe were determined by Darcy's general formula, with $C = 0.00033$ for wood and $= 0.00066$ for iron pipe, checking by Kutter's formula, with $n = 0.01$ for wood and $= 0.012$ for iron. These coefficients were taken as conservative and on the safe side, and such they proved to be. It was desired that the line should carry not less than 5 sec-ft. to Nogal and half as much beyond.

Velocities.—The pipe line from Bonito Creek to the Nogal Reservoir affords excellent conditions for velocity and capacity measurements, there being no distribution service from it. Beginning at the creek, it consists of 12 700 ft. of 10-in. wood pipe, with a hydraulic grade of 0.03338, followed by 48 000 ft. of 16-in. wood pipe, with a hydraulic grade of 0.0030625, ending on the south rim of the Nogal Reservoir. There is an open stand-pipe where the two pipes and grades join.

When this section of the line was laid, the last car of 16-in. pipe was late in arriving and, as it was desirable to get water into the reservoir as soon as possible, 500 ft. of 10-in. pipe were laid in the lower part of the 16-in. line, near the reservoir, as indicated on Fig. 2, which

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shows the hydraulic grades and the pipe diameters of this section of the line.

When the first two velocity measurements, of March 10th and 31st, 1908, described below, were made (after the line had been put into service on February 20th, 1908), the 500 ft. of 10-in. pipe were still in the 16-in. line, and the hydraulic grade was defined by the solid line, *A B C D E,* Fig. 2.

When the third measurement, of May 12th, 1909, also described below, was made, the 10-in. pipe had been replaced by 16-in. pipe, and the hydraulic grade was defined by the solid line, *A B E.*

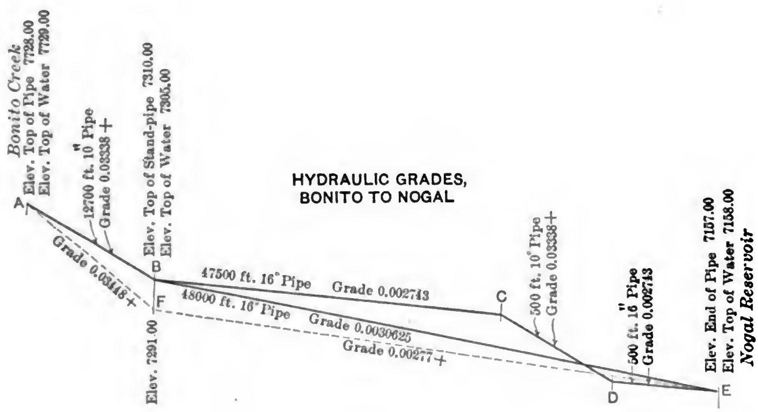

Fig. 2.

The dotted line, *A F E,* is the approximate theoretical position which the grade, *A B C D E,* should have assumed when the 500 ft. of 10-in. pipe were taken out of the 16-in. line. On the contrary, it took the position of the grade line, *A B E.*

During the interval between March, 1908, and May, 1909, the water came to overflow from the stand-pipe at *B,* when the line was running under full pressure, indicating an increase of capacity in the 10-in. pipe greater than a corresponding increase in the 16-in. The alignment of the 10-in. line, vertically and horizontally, is more regular and uniform than the 16-in. line. The latter has many abrupt curves and bends, vertically and horizontally. It crosses nine sharp ridges and dips under as many deep arroyos. This introduces a fixed element of frictional resistance which does not decrease with the increasing

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smoothness of the interior surface of wood pipe, and probably accounts for the higher resistance of the 16-in. line.

From Fig. 2 it appears that, while the 10-in. line had an initial coefficient of roughness slightly greater than 0.009 and now equal to it, the 16-in. line had one equal at first but now slightly less than 0.01.

The line from Bonito Creek to Nogal Reservoir was to have a capacity of 5 sec-ft. Referring to the profile, it was determined that for the hydraulic grade of 33⅓ ft. per 1 000 ft., a 10-in. pipe was necessary, and that a 16-in. pipe was required for the grade of 3 ft. per 1 000 ft.

Test No. 1.—On March 10th, 1908, a quantity of bran was poured into the upper end of the 10-in. pipe at *A* (Fig. 2), and the time of its appearance at the lower end of the 16-in. pipe at *E* was noted. The time was 3 hours and 50 min.

This gave:

Area	of 10-in.	pipe	=	0.5454 sq. ft.
"	" 16 "	"	=	1.3960 " "
Length	" 10 "	"	=	13 200 ft.
"	" 16 "	"	=	47 500 "
Time,			=	13 800 sec.

Let x = velocity of flow in 16-in. pipe, in feet per second, then $2.56\,x$ = velocity of flow in 10-in. pipe, in feet per second.

From which:

$$\frac{13\,200}{2.56\,x} + \frac{47\,500}{x} = 13\,800$$

$$x = 3.805$$

$$\text{and } 2.56x = 9.740$$

The discharge is:

For the 16-in. pipe, $1.396 \times 3.805 = 5.31$ cu. ft. per sec.; and, for the 10-in. pipe, $0.5454 \times 9.74 = 5.31$ cu. ft. per sec.

The question arose as to whether or not the particles of bran in the water traveled as fast as the water flowed. It was also desired to check by observation the relative velocities in the two pipes, as above deduced.

Test No. 2.—To determine these points, a second test was made, on March 31st, 1908, twenty days after the first one. In this test, green aniline, red potassium permanganate, and bran were used. An

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observer was placed at the end of the 10-in. line at B (Fig. 2), and, by letting a small quantity of water run from a relief-valve there, he was able to note the time of the appearance of the colors and the bran.

The green was started in the upper end of the 10-in. pipe, at A (Fig. 2), at 8.30 A. M. It appeared at B in 22 min., and at E in 3 hours and 52 min.

The red was started at 8.45 A. M. It reached B in 21½ min., but it was so faded that the time of its appearance at E could not be noted exactly.

The bran was started at 9.00 A. M. It reached B in 22 min., and appeared at E in 3 hours and 51 min.

From the average of these figures, the velocities were:

In the 16-in. pipe,	3.792	ft. per sec.	
" " 10 " "	9.695	" " "	

and the discharges were:

In the 10-in. pipe,	5.287	cu. ft. per sec.
" " 16 " "	5.293	" " " "

The application of the equation for equalized relative velocities, as in the first test, gives:

Velocity in 16-in. pipe	= 9.705
" " 10 " "	= 3.791
Discharge of 16 " "	= 5.292
" " 10 " "	= 5.293

These last figures would check exactly, except for dropping figures in the fourth decimal place.

The results of these two tests, considering that 20 days elapsed between them, are in very close agreement, and establish the fact that bran is an accurate medium of measurement.

Test No. 3.—The 500 ft. of 10-in. pipe in the 16-in. line near the reservoir (Fig. 2) were replaced by 16-in. pipe in the summer of 1908.

On May 12th, 1909, green aniline was started through the pipe at A at 11.00 A. M., 11.30 A. M., and 12.00 M. In each case it appeared at E in 3 hours and 31 min. This time is 20 min. less than that observed in the tests of the previous year, and is due to the removal of the 10-in. pipe from the 16-in. line and to the increasing smoothness of the interior surface of the pipe.

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The relative velocities and discharges under the third test, using the nomenclature of the first and correcting the lengths of pipe on account of the removal of the 10-in. pipe near the reservoir, are:

$$\frac{48\,000}{x} + \frac{12\,700}{2.56\,x} = 12\,660$$

$$x = 4.183$$

$$\text{and } 2.56\,x = 10.708$$

and the discharges are:

From the 10-in. pipe = 5.840 cu. ft. per sec.
" " 16 " " = 5.839 " " " "

Coefficients.—On May 12th, 1909, the 10-in. line was working on a grade of 0.03338, and, with $n = 0.009$, C should have been 131. It was actually 138, making $n = 0.00866$. The 16-in. line was working on a grade of 0.0030625, and, with $n = 0.009$, C should have been 145. It was actually 141, making $n = 0.0092$.

Referring to the estimated hydraulic grade between Coyote and Corona (Plate V), the coefficients, 0.01 and 0.012, were used for wood and iron, respectively, on which basis, the maximum pressure at Coyote was expected to be 304 lb. and, at Luna, 310 lb. per sq. in. The actual maximum at Coyote, with pumps at full normal speed, was 270 lb., and, at Luna, 278 lb., indicating that the values of the coefficients taken were too high. This checks with the tests between Bonito and Nogal.

Of course, the iron pipe will increase in roughness, and, in time the pumping pressure will approach the calculated amount. The interior of the iron pipe now has a smooth coat of asphalt.

Pipe Breakage.—The breakage or damage to the wood pipe in shipment occurred on the ends, the tenons being most exposed to injury from shifting in the cars. The damage due to the shipment and handling of the Elmira pipe was 1% and one-half as much for the Bay City pipe. Less than 6 pieces out of 100 000 laid, have had to be removed from the trench.

The iron pipe came from Chattanooga, and was badly handled in transit. Much of it was transferred en route, and 6% was broken when received. The breaks were generally cracks of the spigot end. Of this broken pipe, practically all was cut and laid. The average cut was about 16 in. from the spigot end of 533 pieces. This cut pipe has caused no trouble in the trench.

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At least 27 pieces of cracked pipe got past the field inspectors and into the trench. This cracked pipe began blowing out at a pressure of 50 lb., and continued until the full normal pumping pressure was reached, when the breaks suddenly ceased. These pipes were broken out at the rate of 1 or 2 per day, with an occasional day between breaks. A 24-hour work-train service was maintained. The pipe gang soon became skilled, and could put in a new section of pipe in from 4 to 6 hours. Each break generally caused an interruption of about 6 hours to the pumps on the section where it occurred. The best record was 3 hours and 50 min. from the stopping to the starting of the pumps. This strenuous life lasted 30 days. Most of these breaks were in or near the middle of the pipe. Evidently, the field inspectors were not expecting cracks in that locality. An inspection usually indicated that the pipe had been struck by the bell of another one in the vicinity of the break.

All pipes were lifted from the car carefully and laid down at the trench along the track in a single movement by a logging crane, and were not broken in such handling.

Three breaks only have been reported as due to defective metal or casting. No break of a sound shell of full thickness has been found.

Trenching.—Deep frosts are unknown in this section. The pipe was laid so that the top was about 1 ft. below the surface of the ground. The trenching was a simple matter. Part of the work between Bonito and the railway on the Carrizozo plain was done by Buckeye ditchers. All other ditching was done by a railroad plow followed by pick and shovel, or by the two latter tools only. The ditcher could open 2 000 ft. of trench per day, but averaged about 500. The plow and 35 men could open 3 500 ft. A chain about 6 ft. long separated the end of the plow beam and the double tree. In this way the trench was plowed to the bottom. Two mules, two men, and a scraper could back-fill 3 500 ft. per day.

Pipe Laying.—Between Bonito and the railway, one gang of ten men could lay 4 000 ft. of 12-in. pipe per day. The average was much less, owing to a variety of causes. At the end, the railway company added to the contractor's force, and laid the last 10 miles of pipe in 7 days, there being a half dozen separate gangs at work.

Along the railway, the day's record on wood pipe was 4 000 ft. of 11-in., 6 200 ft. of 7½-in. and 8 345 ft. of 3½-in. pipe laid by a gang

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of eight men after the pipe was distributed along the trench. These eight men, of whom five were Americans, laid 76 miles of pipe, and became expert. Their operation was like the working of a clock.

On the 12-in. iron pipe, the regular day's work was 96 joints, or 1 152 ft. of pipe laid and caulked. The record was 1 644 ft. Two gangs laid 101 300 lin. ft. in 60 days. Such a gang consisted of 1 foreman, 1 inspector, 8 caulkers, 4 yarners, 1 melter, 1 pourer, 1 helper, and 10 men putting pipe into the trench.

Cost Data.—The pipe from Bonito to the railway was laid by contract. The price was 18 cents per lin. ft. laid and back-filled from the railway to the Nogal Reservoir, and 28 cents from Nogal to Bonito. In addition, 50 cents per ton per mile was paid for hauling pipe, and extra compensation for setting valves. From Coyote, east along the railway, the work was done by the railway company under the writer's direction.

The total cost of laying 384 300 ft. of wood pipe, from 11 to 3½ in. in diameter, was $18 156.77, or 4.72 cents per ft., divided as follows:

Ditching	$0.0249
Laying	0.0113
Back-filling	0.0110
Total	$0.0472

This includes unloading from the cars. Train service cost ½ cent per ft. additional.

The pipe gang, including back-filling, consisted of 1 foreman, at $100 per month, one assistant foreman at $75, and about 30 Mexicans at $1 per day. The rates were the same in the ditching gang. The plow team cost $6 per day.

Including all general expense, the cost does not exceed 6 cents per lin. ft.

The cost of laying 101 300 ft. of 12-in. cast-iron pipe was $23 826.67, or 23.5 cents per ft., divided as follows:

Ditching	$0.0249
Laying	0.1180
Back-filling	0.0110
Lead	0.0790
Oakum	0.0014
Total	$0.2343

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This includes train service and unloading pipe, but nothing for tools. The foreman and inspector received $100 per month, the caulkers, $3; pourer, $3; melter, $2.50; 2 pipe-men, $2, and laborers, $1 per day. Professional caulkers wanted $5 per day. Carpenters, blacksmiths, and boiler-makers made good caulkers; their work is standing perfectly under a 275-lb. service.

The cost of the pumping plants complete per horse-power is as follows:

Pumps	$79.00
Boilers	18.70
Building	41.70
Total	$139.40 per h.p.

The approximate cost per million gallons of storage capacity is as follows:

Nogal	Storage	Reservoir	$103.00
Carrizozo	Service	"	3 040.00
Coyote	"	"	2 880.00
Luna	"	"	3 480.00
Corona	"	"	2 720.00

To cover general expense, 3% should be added to all the costs above given. The costs per foot of pipe-laying include the setting of all specials, valves, and stand-pipes. The difference of cost in laying 11-in. and 3½-in. wood pipe is not nearly as great as the difference in diameter or the total quantity laid on record days. While the record is 4 000 ft. and 8 345 ft., the 76 miles of pipe of all diameters were laid in a total time, including all delays, of 223 days, or an average of only 1 723 ft. per day. The cost of the 11-in. pipe is covered by 7 cents per ft. The pipe was laid by a single gang as fast as it was received from the factory.

The reduction from 7 to 3½ in. at Mile 230 (Plate V) is on account of delivering water to the Santa Fé's new transcontinental low-grade line which crosses the El Paso and Southwestern Railway at Vaughn, and has a division point there. On its adjacent divisions, the Santa Fé had the same trouble with local waters which compelled the El Paso and Southwestern to find a better supply. The Bonito water is conducted to and used at points 160 miles from its origin on Bonito Creek.

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DISCUSSION

Mr. Smith. G. E. P. Smith, Assoc. M. Am. Soc. C. E. (by letter).—The author has done great service to the West in demonstrating the practicability of transporting small water supplies to great distances.

Close association with the desert is required to appreciate fully its waterless condition. For most of the year there are no living waters on the surface. As a rule, ground-waters are concentrated beneath very limited areas of valley land. The great masses of valley fill in some places are underdrained to great depths and in other places are so compacted and cemented as to be impervious. Wells sometimes are driven from 1 000 to 2 000 ft., without securing any supply at all. Moreover, desert ground-waters are often exceedingly hard or alkaline, and, therefore, are unfit for many uses.

In going to the high mountains for a supply, the author has struck a principle of wide application. In many of the mountains of the Southwest there are springs and small streams of excellent water. Often, as in the case discussed, very little storage is required. These streams, however, are absorbed or disappear before reaching even the mouths of the cañons, and the problem has been to convey the water to distant cities and mining camps at reasonable cost. There are several cities in Arizona now possessing pumped water supplies, which have possible gravity supplies of superior quality. The writer believes that ultimately the gravity supplies will replace the pumping plants.

In the Bonita pipe line, wood-stave pipe was used for the gravity sections. In other localities, where the grade of the line is very uniform, as would be the case down a typical clinoplain, cement pipe is deserving of consideration. It would cost no more than wood stave, would be more durable, and, furthermore, it need have no greater leakage. Its cost, however, increases rapidly when built to withstand high pressures.

The use of bran for determining velocities is of interest. The results are in close accord with those obtained from the weir measurements. In the measurement of ground-water velocities by means of salts in solution, it is found that the velocities of different filaments of waters are extremely variable, and a quart of salt solution, after moving forward a few feet, is widely dispersed. It would be of value to know to what extent the bran was distributed during its 4-hour journey through the pipe line, and during how many minutes it was being discharged at the lower end of the line. Was the first appearance, or the average time of appearance, accepted for computing the velocity of flow?

Mr. Allen. Kenneth Allen, M. Am. Soc. C. E. (by letter).—From its lightness, toughness, flexibility, and the facility with which it can be laid, wood pipe has manifest advantages for use in inaccessible places and

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where handling is difficult; loss in transportation is almost negligible, it will stand much unequal settlement without cracking, and ordinary leaks are easily repaired. Mr. Allen.

The coating of the bands is of such great importance that it should be inspected very thoroughly, in order to remedy defects before the back-filling is done. The writer has found Durable Metal Coating an excellent preservative. Bands coated with this preparation were buried in a salt marsh, and, after a year, the metal was found intact and the coating fresh and elastic. This coating, however, does not adhere very firmly to a smooth metal surface, so that, with careless handling, patches may become rubbed or torn off.

There is no advantage in coating the surface of the pipe. To prevent decay, such pipe should carry water under pressure or be laid in a saturated soil, so that the wood of which it is made will always be saturated, and coating the wood may interfere with this. Under these conditions the life of such pipe is not known, but it is evidently very great. Large quantities of wood pipe have been removed from trenches in Boston, New York City, Philadelphia, Baltimore, and elsewhere, usually in perfectly sound condition. It was commonly made of logs of spruce, yellow pine, or oak, from 12 to 18 ft. long, 12 to 24 in. in diameter, and with a bore from 3 to 6 in. in diameter. Some 6-in. pipe taken up in Philadelphia had an external diameter of 30 in. The ends were usually bound with wrought-iron collars, and adjacent lengths were connected by an iron thimble driven into the end of each piece.

A few years ago the writer took up more than 2 000 ft. of wood pipe of this kind, which had been laid in saturated soil about a century earlier. It was of Southern pine logs, about 16 in. in diameter, 14½ ft. long, and had a 5-in. bore. Joints were made with tapering cast-iron ferrules 9 in. long, and connections to smaller service pipes were made with similar but smaller ferrules of cast brass. The wood was apparently as sound as when it was first laid.

The use of flat iron for wrapping or banding pipe is believed to be wrong in principle. Round iron furnishes the requisite strength with the least exposure to corrosion, and ensures a more perfect contact with the wood.

In a 42-in. stave pipe laid by the writer for the Water Department of Atlantic City, N. J., the lumber used was Washington fir, cypress having been found difficult to procure in sufficient quantity, and redwood being more costly and no better. In this, his experience coincided with that of the author. Cedar was considered, but could not be obtained in sufficient lengths or quantity, and long-leaf pine which would have passed the somewhat rigid specifications would have been difficult to secure. It is believed, however, that there is a field at least for long-leaf pine for such construction. Washington fir was found admirable in every respect, and was moderate in cost at that time.

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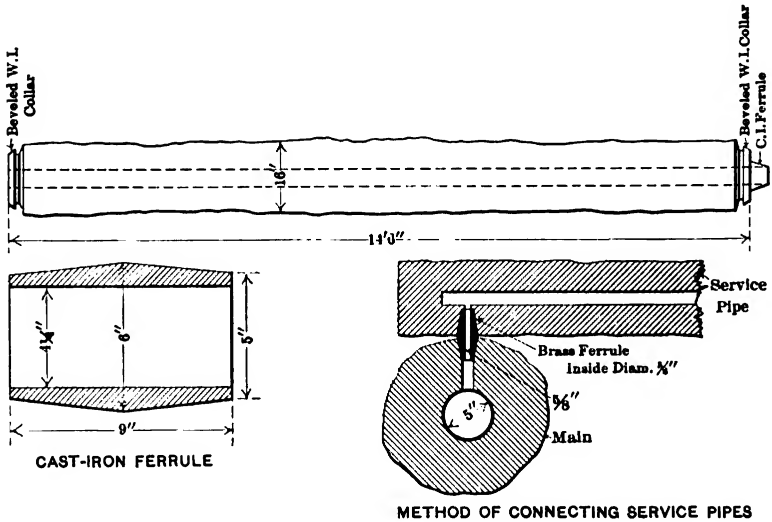
Mr. Allen. The bands were bent in the field, and, after heating in an oven for about 3 min., were dipped in bunches of five into a kettle of melted mineral rubber at a temperature of about 400° Fahr., and then hung up for the coating to harden. This took place rapidly, as the work was done in winter. If the band were wound spirally, the coating would have to be done in the shop, but field coating is preferable, as it avoids injury to the coating during transportation.

An advantage of wood pipe for conveying water is its low coefficient of friction. The results obtained by the author ($n = 0.00866$ to 0.0092) appear to be very low as compared with determinations made for wood-



DETAILS OF OLD WOOD PIPE.

FIG. 3.

stave pipe. Kutter's coefficient for the latter varies from 0.0096 in the case of the 30-in. pipe at Denver,* to from 0.012 to 0.015 as determined by Messrs. Marx, Wing, and Hoskins for the 72-in. pipe of the Pioneer Power Plant of Ogden.† Probably 0.011 would be a fairly safe figure to use in designing new work.

Mr. Campbell. J. L. CAMPBELL, M. AM. SOC. C. E. (by letter).—Referring to Mr. Smith's question about the velocity measurements by bran, the first appearance of the bran and the colors was taken because the intervals of time given thereby were in close accord among themselves and with the weir measurements. The time from the first trace of bran or color until final disappearance varied between 15 and 20 min. Bran

* *Transactions*, Am. Soc. C. E., Vol. XXXVI, p. 26.

† *Journal*, New England Water Works Assoc., Vol. XXII, p. 279.

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Mr. Campbell

in abundance or pronounced color showed in 2 min. after the first appearance, while the disappearance or fading was noticeable after a period of from 7 to 10 min. It required 2½ min. to get the bran or colors into the intake at the head of the line and leave the water clear.

Mr. Allen refers to the bored wood pipe laid many years ago in Eastern cities. The writer's experience indicates that a bored pipe will not deliver as much water as a planed stave pipe, on account of the greater interior roughness of the former.

Referring to the profile, the 8½-in. pipe between Corona and Duran had a theoretical capacity of 744 000 gal. per day. A recent test showed it to be delivering water at the rate of 759 000 gal. per day.

The 3½-in. pipe between Vaughn and Pastura had a theoretical capacity of 84 000 gal. per day. It delivers only 65 000 gal. per day. There are 5 miles of bored pipe on the upper end of this section. Pressure gaugings show a hydraulic gradient in excess of the theoretical on the bored pipe, whereas the stave pipe on the lower end carries the 65 000 gal. on a flatter gradient than the theoretical one.

Experience on this pipe line indicates that $n = 0.009$, in Kutter's formula, closely approximates the capacity of planed wood stave pipes of 8 to 16 in. in diameter. The writer favors the use of 0.01 as conservative and economical.

With equal exposure to corrosion, the round band is undoubtedly the better, but the flat band has the advantage of being completely buried in the protective coat of the particular kind of wood pipe under consideration.

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AMERICAN SOCIETY OF CIVIL ENGINEERS
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TRANSACTIONS

Paper No. 1171

FEDERAL INVESTIGATIONS OF MINE ACCIDENTS, STRUCTURAL MATERIALS, AND FUELS.*

By Herbert M. Wilson, M. Am. Soc. C. E.

With Discussion by Messrs. Kenneth Allen, Henry Kreisinger, Walter O. Snelling, A. Bartoccini, H. G. Stott, B. W. Dunn, and Herbert M. Wilson.

Introduction.

The mine disaster, which occurred at Cherry, Ill., on November 13th, 1909, when 527 men were in the mine, resulting in the entombment of 330 men, of whom 310 were killed, has again focused public attention on the frequent recurrence of such disasters and their appalling consequences. Interest in the possible prevention of such disasters, and the possible means of combating subsequent mine fires and rescuing the imprisoned miners, has been heightened as it was not even by the series of three equally extensive disasters which occurred in 1907, for the reason that, after the Cherry disaster, 20 men were rescued alive after an entombment of one week, when practically all hope of rescuing any of the miners had been abandoned.

This accident, occurring, as it does, a little more than 1½ years after the enactment of legislation by Congress instructing the Director of the United States Geological Survey to investigate the causes and possible means of preventing the loss of life in coal-mining

* Presented at the meeting of April 20th, 1910.

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operations, makes this an opportune time to review what has been done by the Geological Survey during this time, toward carrying out the intent of this Act.

It may be stated with confidence, that had such a disaster occurred a year or more ago, all the entombed men must have perished, as it would have been impossible to enter the mine without the protection afforded by artificial respiratory apparatus. Moreover, but for the presence of the skilled corps of Government engineers, experienced by more than a year's training in similar operations in more than twenty disasters, the mine would have been sealed until the fire had burned out, and neither the dead, nor those who were found alive, would have been recovered for many weeks. In the interval great suffering and loss would have been inflicted on the miners, because of enforced idleness, and on the mine owners because of continued inability to re-open and resume operations.

Character of the Work.—The United States Geological Survey has been engaged continuously since 1904 in conducting investigations relating to structural materials, such as stone, clay, cement, etc., and in making tests and analyses of the coals, lignites, and other mineral fuel substances, belonging to, and for the use of, the Government.

Incidentally, the Survey has been considering means to increase efficiency in the use of these resources as fuels and structural materials, in the hope that the investigations will lead to their best utilization.

These inquiries attracted attention to the waste of human life incident to the mining of fuel and its preparation for the market, with the result that, in May, 1908, provision was made by Congress for investigations into the causes of mine explosions with a view to their prevention.

Statistics collected by the Geological Survey show that the average death rate in the coal mines of the United States from accidents of all kinds, including gas and dust explosions, falls of roof, powder explosions, etc., is three times that of France, Belgium, or Germany. On the other hand, in no country in the world are natural conditions so favorable for the safe extraction of coal as in the United States. In Belgium, foremost in the study of mining conditions, a constant reduction in the death rate has been secured, and from a rate once nearly as great as that of the United States, namely, 3.28 per thousand, in the period 1851-60, it had been reduced to about 2 per thousand in

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the period 1881-90; and in the last decade this has been further reduced to nearly 1 per thousand. It seems certain, from the investigations already made by the Geological Survey, that better means of safeguarding the lives of miners will be found, and that the death rate from mine accidents will soon show a marked reduction.

Other statistics collected by the Geological Survey show that, to the close of 1907, nearly 7 000 000 000 tons of coal had been mined in the United States, and it is estimated that for every ton mined nearly a ton has been wasted, 3 500 000 000 tons being left in the ground or thrown on the dump as of a grade too low for commercial use. To the close of 1907 the production represents an exhaustion of somewhat more than 10 000 000 000 tons of coal. It has been estimated that if the production continues to increase, from the present annual output of approximately 415 000 000 tons, at the rate which has prevailed during the last fifty years, the greater part of the more accessible coal supply will be exhausted before the middle of the next century.

The Forest Service estimates that, at the present rate of consumption, renewals of growth not being taken into account, the timber supply will be exhausted within the next quarter of a century. It is desirable, therefore, that all information possible be gained regarding the most suitable substitutes for wood for building and engineering construction, such as iron, stone, clay products, concrete, etc., and that the minimum proportion in which these materials should be used for a given purpose, be ascertained. Exhaustion, by use in engineering and building construction, applies not only to the iron ore, clay, and cement-making materials, but, in larger ratio, to the fuel essential to rendering these substances available for materials of construction. Incidentally, investigations into the waste of structural materials have developed the fact that the destructive losses, due to fires in combustible buildings, amount to more than $200 000 000 per annum. A sum even greater than this is annually expended on fire protection. Inquiries looking to the reduction of fire losses are being conducted in order to ascertain the most suitable fire-resisting materials for building construction.

Early in 1904, during the Louisiana Purchase Exposition, Congress made provision for tests, demonstrations, and investigations concerning the fuels and structural materials of the United States. These investigations were organized subsequently as the Technologic Branch

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of the United States Geological Survey, under Mr. Joseph A. Holmes, Expert in Charge, and the President of the United States invited a group of civilian engineers and Chiefs of Engineering Bureaus of the Government to act as a National Advisory Board concerning the method of conducting this work, with a view to making it of more immediate benefit to the Government and to the people of the United States. This Society is formally represented on this Board by C. C. Schneider, Past-President, Am. Soc. C. E., and George S. Webster, M. Am. Soc. C. E. Among representatives of other engineering societies, or of Government Bureaus, the membership of the National Advisory Board includes other members of this Society, as follows: General William Crozier, Frank T. Chambers, Professor W. K. Hatt, Richard L. Humphrey, Robert W. Hunt, H. G. Kelley, Robert W. Lesley, John B. Lober, Hunter McDonald, and Frederick H. Newell.

In view, therefore, of the important part taken both officially and unofficially by members of this Society in the planning and organization of this work, it seems proper to present a statement of the scope, methods, and progress of these investigations. Whereas the Act governing this work limits the testing and investigation of fuels and of structural materials to those belonging to the United States, the activities of the Federal Government in the use of these materials so far exceeds that of any other single concern in the United States, that the results cannot but be of great value to all engineers and to all those engaged in engineering works.

Mine Accidents Investigations.

Organization, and Character of the Work.—The mine rescue investigations, carried on at the Federal testing station, at Pittsburg, Pa., include five lines of attack:

1.—Investigations in the mines to determine the conditions leading up to mine disasters, the presence and the relative explosibility of mine gas and coal dust, and mine fires and means of preventing and combating them.

2.—Tests to determine the relative safety, or otherwise, of the various explosives used in coal mining, when ignited in the presence of explosible mixtures of natural gas and air, or coal dust, or of both.

3.—Tests to determine the conditions under which electric equipment is safe in coal-mining operations.

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4.—Tests to determine the safety of various types of mine lights in the presence of inflammable gas, and their accuracy in detecting small percentages of mine gas.

5.—Tests of the various artificial breathing apparatus, and the training of miners and of skilled mining engineers in rescue methods.

The first four of these lines of investigation have to do with preventive measures, and are those on which ultimately the greatest dependence must be placed. The fifth is one in which the result seems at first to be the most apparent. It has to do, not with prevention, but with the cure of conditions which should not arise, or, at least, should be greatly ameliorated.

During the last 19 years, 28 514 men have been killed in the coal-mining industries.* In 1907 alone, 3 125 men lost their lives in coal mines, and, in addition, nearly 800 were killed in the metal mines and quarries of the country. Including the injured, 8 441 men suffered casualties in the mines in that year. In every mining camp containing 1 000 men, 4.86 were taken by violent death in that year. In the mining of coal in Great Britain, 1.31 men were killed in every 1 000 employed in the same year; in France, 1.1; in Belgium, 0.94, or less than 1 man in every 1 000 employed. It is thus seen that from three to four times as many men are being killed in the United States as in any European coal-producing country. This safer condition in Europe has resulted from the use of safer explosives, or the better use of the explosives available; from the reduction in the use of open lights; from the establishment of mine rescue stations and the training with artificial breathing apparatus; and from the adoption of regulations for safeguarding the lives of the workmen.

The mining engineering field force of the Geological Survey, at the head of which is Mr. George S. Rice, an experienced mining and consulting engineer, has already made great progress in the study of underground mining conditions and methods. Nearly all the more dangerous coal mines in the United States have been examined; samples of gas, coal, and dust have been taken and analyzed at the chemical laboratories at Pittsburg; extended tests have been made as to the explosibility of various mixtures of gas and air; as to the explosibility of dust from various typical coals; as to the chemical composition and

* "Coal Mine Accidents," by Clarence Hall and Walter O. Snelling. Bulletin No. 333, U. S. Geological Survey, Washington, D. C.

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physical characteristics of this dust; the degree of fineness necessary to the most explosive conditions; and the methods of dampening the dust by water, by humidifying, by steam, or of deadening its explosibility by the addition of calcium chloride, stone dust, etc. A bulletin outlining the results thus far obtained in the study of the coal-dust problem is now in course of publication.*

After reviewing the history of observations and experiments with coal dust carried on in Europe, and later, the experiments at the French, German, Belgian, and English explosives-testing stations, this bulletin takes up the coal-dust question in the United States. Further chapters concern the tests as to the explosibility of coal dust, made by the Geological Survey, at Pittsburg; investigations, both at the Pittsburg laboratory and in mines, as to the humidity of mine air. There is also a chapter on the chemical investigations into the ignition of coal dust by Dr. J. C. W. Frazer, of the Geological Survey. The application of some of these data to actual mine conditions in Europe, in the last year, is treated by Mr. Axel Larsen; the use of exhaust steam in a mine of the Consolidation Coal Company, in West Virginia, is discussed by Mr. Frank Haas, Consulting Engineer; and the use of sprays in Oklahoma coal mines is the subject of a chapter by Mr. Carl Scholz, Vice-President of the Rock Island Coal Mining Company.

An earlier bulletin setting forth the literature and certain mine investigations of explosive gases and dust,† has already been issued. After treating of methods of collecting and analyzing the gases found in mines, of investigations as to the rate of liberation of gas from coal, and of studies on coal dust, this bulletin discusses such factors as the restraining influence of shale dust and dampness on coal-dust explosions. It then takes up practical considerations as to the danger of explosions, including the relative inflammability of old and fresh coal dust. The problems involved are undergoing further investigation and elaboration, in the light of information already gathered.

Permissible Explosives.—The most important progress in these tests and investigations has been made in those relating to the various explosives used in getting coal from mines. Immediately upon the enactment of the first legislation, in the spring of 1908, arrangements

* "The Explosibility of Coal Dust," by George S. Rice and others. Bulletin No. * * *, U. S. Geological Survey.

† "Notes on Explosives, Mine Gases and Dusts," by Rollin Thomas Chamberlin. Bulletin No. 383, U. S. Geological Survey, 1909.

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were perfected whereby the lower portion of the old Arsenal grounds belonging to the War Department and adjacent to the Pennsylvania Railroad, on the Alleghany River, at 40th and Butler Streets, Pittsburg, Pa., were transferred to the Interior Department for use in these investigations. Meantime, in anticipation of the appropriation, Mr. Clarence Hall, an engineer experienced in the manufacture and use of explosives, was sent to Europe to study the methods of testing explosives practiced at the Government stations in Great Britain, Germany, Belgium, and France. Mr. Joseph A. Holmes also visited Europe for the purpose of studying methods of ameliorating conditions in the mines. Three foreign mining experts, the chiefs of investigating bureaus in Belgium, Germany, and England, spent three months studying conditions in the United States at the invitation of the Secretary of the Interior, to whom they submitted a valuable report.*

Under the supervision of the writer, Chief Engineer of these investigations, detailed plans and specifications had been prepared in advance for the necessary apparatus and the transformation of the buildings at Pittsburg to the purposes of this work. It was possible, therefore, to undertake immediately the changes in existing buildings, the erection of new buildings, the installation of railway tracks, laboratories, and the plumbing, heating, and lighting plant, etc. This work was carried on with unusual expedition, under the direction of the Assistant Chief Engineer, Mr. James C. Roberts, and was completed within a few months, by which time most of the apparatus was delivered and installed.

One building (No. 17) is devoted to the smaller physical tests of explosives. It was rendered fire resistant by heavily covering the floors, ceiling, and walls with cement on metal lath, and otherwise protecting the openings. In it are installed apparatus for determining calorific value of explosives, pressure produced on ignition, susceptibility to ignition when dropped, rate of detonation, length and duration of flame, and kindred factors. Elsewhere on the grounds is a gallery of boiler-steel plate, 100 ft. long and more than 6 ft. in diameter, solidly attached to a mass of concrete at one end, in which is embedded a cannon from which to discharge the explosive under test, and open at the other end, and otherwise so constructed as to

* "Prevention of Mine Explosions," by Victor Watteyne, Carl Meissner, and Arthur Desborough. Bulletin No. 369, U. S. Geological Survey.

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FIG 1.—EXPLOSION FROM COAL DUST IN GAS AND DUST GALLERY NO. 1.

FIG. 2. MINE GALLERY NO. 1.

FIG. 3.—BALLISTIC PENDULUM.

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simulate a small section of a mine gallery (Fig. 2, Plate VI). The heavy mortar pendulum, for the pendulum test for determining the force produced by an explosive, is near by, as is also an armored pit in which large quantities of explosive may be detonated, with a view to studying the effects of magazine explosions, and for testing as to the rate at which ignition of an explosive travels from one end to the other of a cartridge, and the sensitiveness of one cartridge to explosion by discharge of another near by.

In another building (No. 21), is a well-equipped chemical laboratory for chemical analyses and investigations of explosives, structural materials, and fuels.

Several months were required to calibrate the various apparatus, and to make analyses of the available natural gas to determine the correct method of proportioning it with air, so as to produce exact mixtures of 2, 4, 6, or 8% of methane with air. Tests of existing explosives were made in air and in inflammable mixtures of air and gas, with a view to fixing on some standard explosive as a basis of comparison. Ultimately, 40% nitro-glycerine dynamite was adopted as the standard. Investigative tests having been made, and the various factors concerning all the explosives on the market having been determined, a circular was sent to all manufacturers of explosives in the United States, on January 9th, 1909, and was also published in the various technical journals, through the associated press, and otherwise.

On May 15th, 1909, all the explosives which had been offered for test, as permissible, having been tested, the first list of permissible explosives was issued, as given in the following circular:

"EXPLOSIVES CIRCULAR NO. 1.

"DEPARTMENT OF THE INTERIOR.

"UNITED STATES GEOLOGICAL SURVEY.

"MAY 15, 1909.

"LIST OF PERMISSIBLE EXPLOSIVES.

"Tested prior to May 15, 1909.

"As a part of the investigation of mine explosions authorized by Congress in May, 1908, it was decided by the Secretary of the Interior that a careful examination should be made of the various explosives used in mining operations, with a view to determining the extent to

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which the use of such explosives might be responsible for the occurrence of these disasters.

"The preliminary investigation showed the necessity of subjecting to rigid tests all explosives intended for use in mines where either gas or dry inflammable dust is present in quantity or under conditions which are indicative of danger.

"With this in view, a letter was sent by the Director of the United States Geological Survey on January 9, 1909, to the manufacturers of explosives in the United States, setting forth the conditions under which these explosives would be examined and the nature of the tests to which they would be subjected.

"Inasmuch as the conditions and tests described in this letter were subsequently followed in testing the explosives given in the list below, they are here reproduced, as follows:

"(1) The manufacturer is to furnish 100 pounds of each explosive which he desires to have tested; he is to be responsible for the care, handling, and delivery of this material at the testing station on the United States arsenal grounds, Fortieth and Butler streets, Pittsburg, Pa., at the time the explosive is to be tested; and he is to have a representative present during the tests, who will be responsible for the handling of the packages containing the explosives until they are opened for testing.

"(2) No one is to be present at or to participate in these tests except the necessary government officers at the testing station, their assistants, and the representative of the manufacturer of the explosives to be tested.

"(3) The tests will be made in the order of the receipt of the applications for them, provided the necessary quantity of the explosive is delivered at the plant by the time assigned, of which due notice will be given by the Geological Survey.

"(4) Preference will be given to the testing of explosives that are now being manufactured and that are in that sense already on the market. No test will be made of any new explosive which is not now being manufactured and marketed, until all explosives now on the market that may be offered for testing have been tested.

"(5) A list of the explosives which pass certain requirements satisfactorily will be furnished to the state mine inspectors, and will be made public in such further manner as may be considered desirable.

"TEST REQUIREMENTS FOR EXPLOSIVES.

"The tests will be made by the engineers of the United States Explosives Testing Station at Pittsburg, Pa., in gas and dust gallery No. 1. The charge of explosive to be fired in tests 1, 2, and 3 shall be equal in disruptive power to one-half pound (227 grams) of 40 per cent. nitroglycerin dynamite in its original wrapper, of the following formula:

Nitroglycerin	40
Nitrate of sodium	44
Wood pulp	15
Calcium carbonate	1
	100

"Each charge shall be fired with an electric fuse of sufficient power to completely detonate or explode the charge, as recommended by the manufacturer. The explosive must be in such condition that the chemical and physical tests do not show any unfavorable results. The explosives in which the charge used is less than 100 grams (0.22 pound) will be weighed in tinfoil without the original wrapper.

"The dust used in tests 2, 3, and 4 will be of the same degree of fineness and taken from one mine.*

"TEST 1.—Ten shots with the charge as described above, in its original wrapper, shall be fired, each with 1 pound of clay tamping, at a gallery temperature of 77° F., into a mixture of gas and air containing 8 per cent. of methane and ethane. An explosive will pass this test if all ten shots fail to ignite the mixture.

"TEST 2.—Ten shots with charge as previously noted, in its original wrapper, shall be fired, each with 1 pound of clay tamping at a gallery temperature of 77° F., into a mixture of gas and air containing 4 per cent. of methane and ethane and 20 pounds of bituminous coal dust, 18 pounds of which is to be placed on shelves laterally arranged along the first 20 feet of the gallery, and 2 pounds to be placed near the inlet of the mixing system in such a manner that all or part of it will be suspended in the first division of the gallery. An explosive will pass this test if all ten shots fail to ignite the mixture.

"TEST 3.—Ten shots with charge as previously noted, in its original wrapper, shall be fired, each with 1 pound of clay tamping at a gallery temperature of 77° F., into 40 pounds of bituminous coal dust, 20 pounds of which is to be distributed uniformly on a horse placed in front of the cannon and 20 pounds placed on side shelves in sections 4, 5, and 6. An explosive will pass this test if all ten shots fail to ignite the mixture.

"TEST 4.—A limit charge will be determined within 25 grams by firing charges in their original wrappers, untamped, at a gallery temperature of 77° F., into a mixture of gas and air containing 4 per cent. of methane and ethane and 20 pounds of bituminous coal dust, to be arranged in the same manner as in test 2. This limit charge is to be repeated five times under the same conditions before being established.

"NOTE.—At least 2 pounds of clay tamping will be used with slow-burning explosives.

"WASHINGTON, D. C., *January 9, 1909.*

"In response to the above communication applications were received from 12 manufacturers for the testing of 29 explosives. Of these explosives, the 17 given in the following list have passed all the test requirements set forth, and will be termed permissible explosives.

"Permissible explosives tested prior to May 15, 1909.

Brand.	Manufacturer.
Ætna coal powder A....	Ætna Powder Co., Chicago, Ill.
Ætna coal powder B....	Do.
Carbonite No. 1.........	E. I. Dupont de Nemours Powder Co., Wilmington, Del.

* With a view to obtaining a dust of uniform purity and inflammability.

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Brand.	Manufacturer.
Carbonite No. 2........	E. I. Du Pont de Nemours Powder Co., Wilmington, Del.
Carbonite No. 3........	Do.
Carbonite No. 1 L. F...	Do.
Carbonite No. 2 L. F...	Do.
Coal special No. 1......	Keystone Powder Co., Emporium, Pa.
Coal special No. 2......	Do.
Coalite No. 1..........	Potts Powder Co., New York City.
Coalite No. 2 D........	Do.
Collier dynamite No. 2..	Sinnamahoning Powder Co., Emporium, Pa.
Collier dynamite No. 4..	Do.
Collier dynamite No. 5..	Do.
Masurite M. L. F.......	Masurite Explosive Co., Sharon, Pa.
Meteor dynamite........	E. I. Du Pont de Nemours Powder Co., Wilmington, Del.
Monobel	Do.

"Subject to the conditions named below, a permissible explosive is defined as an explosive which has passed gas and dust gallery tests Nos. 1, 2, and 3 as described above, and of which in test No. 4 1½ pounds (680 grams) of the explosive has been fired into the mixture there described without causing an ignition.

"*Provided:*

"1. That the explosive is in all respects similar to the sample submitted by the manufacturer for test.

"2. That double-strength detonators are used of not less strength than 1 gram charge consisting by weight of 90 parts of mercury fulminate and 10 parts of potassium chlorate (or its equivalent), except for the explosive 'Masurite M. L. F.,' for which the detonator shall be of not less strength than 1½ grams charge.

"3. That the explosive, if in a frozen condition, shall be thoroughly thawed in a safe and suitable manner before use.

"4. That the amount used in practice does not exceed 1½ pounds (680 grams) properly tamped.

"The above partial list includes the permissible explosives that have passed these tests prior to May 15, 1909. The announcement of the passing of like tests by other explosives will be made public immediately after the completion of the tests for such explosives.

"A description of the method followed in making these and the many additional tests to which each explosive is subjected, together with the full data obtained in each case, will be published by the Survey at an early date.

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"NOTES AND SUGGESTIONS.

"It may be wise to point out in this connection certain differences between the permissible explosives as a class and the black powders now so generally used in coal mining, as follows:

"(*a*) With equal quantities of each, the flame of the black powder is more than three times as long and has a duration three thousand to more than four thousand times that of one of the permissible explosives, also the rate of explosion is slower.

"(*b*) The permissible explosives are one and one-fourth to one and three-fourths times as strong and are said, if properly used, to do twice the work of black powder in bringing down coal; hence only half the quantity need be used.

"(*c*) With 1 pound of a permissible explosive or 2 pounds of black powder, the quantity of noxious gases given off from a shot averages approximately the same, the quantity from the black powder being less than from some of the permissible explosives and slightly greater than from others. The time elapsing after firing before the miner returns to the working face or fires another shot should not be less for permissible explosives than for black powder.

"The use of permissible explosives should be considered as supplemental to and not as a substitute for other safety precautions in mines where gas or inflammable coal dust is present under conditions indicative of danger. As stated above, they should be used with strong detonators; and the charge used in practice should not exceed 1½ pounds, and in many cases need not exceed 1 pound.

"Inasmuch as no explosive manufactured for use in mining is flameless, and as no such explosive is entirely safe under all the variable mining conditions, the use of the terms 'flameless' and 'safety' as applied to explosives is likely to be misunderstood, may endanger human life, and should be discouraged.

"JOSEPH A. HOLMES,
"*Expert in Charge Technologic Branch.*

"Approved, May 18, 1909:
"GEO. OTIS SMITH,
"*Director.*"

In the meantime, many of the explosives submitted, which heretofore had been on the market as safety explosives, were found to be unsafe for use in gaseous or dusty mines, and the manufacturers were permitted to withdraw them. Their weaknesses being known, as a result of these tests, the manufacturers were enabled to produce similar, but safer, explosives. Consequently, applications for further tests continued to pour in, as they still do, and on October 1st, 1909, a second list of permissible explosives was issued, as follows:

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"EXPLOSIVES CIRCULAR NO. 2.

"DEPARTMENT OF THE INTERIOR.

"UNITED STATES GEOLOGICAL SURVEY.

"OCTOBER 1, 1909.

"LIST OF PERMISSIBLE EXPLOSIVES.

"Tested prior to October 1, 1909.

"The following list of permissible explosives tested by the United States Geological Survey at Pittsburg, Pa., is hereby published for the benefit of operators, mine owners, mine inspectors, miners, and others interested.

"The conditions and test requirements described in Explosives Circular No. 1, issued under date of May 15, 1909, have been followed in all subsequent tests.

"Subject to the provisions named below, a permissible explosive is defined as an explosive which is in such condition that the chemical and physical tests do not show any unfavorable results; which has passed gas and dust gallery tests Nos. 1 and 3, as described in circular No. 1; and of which, in test No. 4, 1½ pounds (680 grams) has been fired into the mixture there described without causing ignition.

"Permissible explosives tested prior to October 1, 1909.

"[Those reported in Explosives Circular No. 1 are marked *.]

Brand.	Manufacturer.
*Ætna coal powder A	Ætna Powder Co., Chicago, Ill.
Ætna coal powder AA	Do.
*Ætna coal powder B	Do.
Ætna coal powder C	Do.
Bituminite No. 1	Jefferson Powder Co., Birmingham, Ala.
Black Diamond No. 3	Illinois Powder Manufacturing Co., St. Louis, Mo.
Black Diamond No. 4	Do.
*Carbonite No. 1	E. I. Du Pont de Nemours Powder Co., Wilmington, Del.
*Carbonite No. 2	Do.
*Carbonite No. 3	Do.
*Carbonite No. 1-L. F.	Do.
*Carbonite No. 2-L. F.	Do.
*Coalite No. 1	Potts Powder Co., New York City.
*Coalite No. 2-D	Do.
*Coal special No. 1	Keystone Powder Co., Emporium, Pa.
*Coal special No. 2	Do.

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Brand.	Manufacturer.
*Collier dynamite No. 2......	Sinnamahoning Powder Manufacturing Co., Emporium, Pa.
*Collier dynamite No. 4......	Do.
*Collier dynamite No. 5......	Do.
Giant A low-flame dynamite..	Giant Powder Co. (Con.), Giant, Cal.
Giant B low-flame dynamite..	Do.
Giant C low-flame dynamite..	Do.
*Masurite M. L. F...........	Masurite Explosives Co., Sharon, Pa.
*Meteor dynamite...........	E. I. Du Pont de Nemours Powder Co., Wilmington, Del.
Mine-ite A.................	Burton Powder Co., Pittsburg, Pa.
Mine-ite B.................	Do.
*Monobel	E. I. Du Pont de Nemours Powder Co., Wilmington, Del.
Tunnelite No. 5.............	G. R. McAbee Powder and Oil Co., Pittsburg, Pa.
Tunnelite No. 6.............	Do.
Tunnelite No. 7.............	Do.
Tunnelite No. 8.............	Do.

"*Provided:*

"1. That the explosive is in all respects similar to sample submitted by the manufacturer for test.

"2. That No. 6 detonators, preferably No. 6 electric detonators (double strength), are used of not less strength than 1 gram charge, consisting by weight of 90 parts of mercury fulminate and 10 parts of potassium chlorate (or its equivalent), except for the explosive 'Masurite M. L. F.,' for which the detonator shall be of not less strength than 1½ grams charge.

"3. That the explosive, if frozen, shall be thoroughly thawed in a safe and suitable manner before use.

"4. That the amount used in practice does not exceed 1½ pounds (680 grams), properly tamped.

"The above partial list includes all the permissible explosives that have passed these tests prior to October 1, 1909. The announcement of the passing of like tests by other explosives will be made public immediately after the completion of the tests.

"With a view to the wise use of these explosives it may be well in this connection to point out again certain differences between the permissible explosives as a class and the black powders now so generally used in coal mining, as follows:

"(*a*) With equal quantities of each, the flame of the black powder is more than three times as long and has a duration three thousand to

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more than four thousand times that of one of the permissible explosives; the rate of explosion also is slower.

"(*b*) The permissible explosives are one and one-fourth to one and three-fourths times as strong and are said, if properly used, to do twice the work of black powder in bringing down coal; hence only half the quantity need be used.

"(*c*) With 1 pound of a permissible explosive or 2 pounds of black powder, the quantity of noxious gases given off from a shot averages approximately the same, the quantity from the black powder being less than from some of the permissible explosives and slightly greater than from others. The time elapsing after firing before the miner returns to the working face or fires another shot should not be less for permissible explosives than for black powder.

"The use of permissible explosives should be considered as supplemental to and not as a substitute for other safety precautions in mines where gas or inflammable coal dust is present under conditions indicating danger. As stated above, they should be used with strong detonators, and the charge used in practice should not exceed 1½ pounds and in many cases need not exceed 1 pound.

"JOSEPH A. HOLMES,
"Expert in Charge Technologic Branch.

"Approved, October 11, 1909.
"H. C. RIZER,
"Acting Director."

The second list contains 31 explosives which the Government is prepared to brand as permissible, and therefore comparatively safe, for use in gaseous and dusty mines. An equally large number of so-called safety powders failed to pass these tests. Immediately on the passing of the tests, as to the permissibility of any explosive, the facts are reported to the manufacturer and to the various State mine inspectors. When published, the permissible lists were issued to all explosives manufacturers, all mine operators in the United States, and State inspectors. The effect has been the enactment, by three of the largest coal-producing States, of legislation or regulations prohibiting the use of any but permissible explosives in gaseous or dusty mines, and other States must soon follow. To prevent fraud, endeavor is being made to restrict the use of the brand "Permissible Explosive, U. S. Testing Station, Pittsburg, Pa.," to only such boxes or packages as contain listed permissible explosives.

As these tests clearly demonstrate, both in the records thereof and visually to such as follow them, that certain explosives, especially

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those which are slow-burning like black powder, or produce high temperatures in connection with comparative slow burning, will ignite mixtures of gas and air, or mixtures of coal dust and air, and cause explosions. The results point out clearly to all concerned, the danger of using such explosives. The remedy is also made available by the announcement of the names of a large number of explosives now on the market at reasonable cost, which will not cause explosions under these conditions. It is believed that when permissible explosives are generally adopted in coal mines, this source of danger will have been greatly minimized.

Explosives Investigations.—Questions have arisen on the part of miners or of mine operators as to the greater cost in using permissible explosives due to their expense, which is slightly in excess of that of other explosives; as to their greater shattering effect in breaking down the coal, and in giving a smaller percentage of lump and a larger percentage of slack; and as to the possible danger of breathing the gases produced.

Observations made in mines by Mr. J. J. Rutledge, an experienced coal miner and careful mining engineer connected with the Geological Survey, as to the amount of coal obtained by the use of permissible and other explosives, tend to indicate that the permissible explosives are not more, but perhaps less expensive than others, in view of the fact that, because of their greater relative power, a smaller quantity is required to do the work than is the case, say, with black powder. On the other hand, for safety and for certainty of detonation, stronger detonators are recommended for use with permissible explosives, preferably electric detonators. These may cost a few cents more per blast than the squib or fuse, but there is no danger that they will ignite the gas, and the difference in cost is, in some measure, offset by the greater certainty of action and the fact that they produce a much more powerful explosion, thus again permitting the use of still smaller quantities of the explosive and, consequently, reducing the cost. These investigations are still in progress.

Concerning the shattering of the coal: This is being remedied in some of the permissible explosives by the introduction of dopes, moisture, or other means of slowing down the disruptive effect, so as to produce the heaving and breaking effect obtained with the slower-burning powders instead of the shattering effect produced by

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dynamite. There is every reason to believe that as the permissible explosives are perfected, and as experience develops the proper methods of using them, this difficulty will be overcome in large measure. This matter is also being investigated by the Survey mining engineers and others, by the actual use of such explosives in coal-mining operations.

Of the gases given off by explosives, those resulting from black powder are accompanied by considerable odor and smoke, and, consequently, the miners go back more slowly after the shots, allowing time for the gases to be dissipated by the ventilation. With the permissible explosive, the miner, seeing no smoke and observing little odor, is apt to be incautious, and to think that he may run back immediately. As more is learned of the use of these explosives, this source of danger, which is, however, inconsiderable, will be diminished. Table 1 gives the percentages of the gaseous products of combustions from equal weights of black powder and two of the permissible explosives. Of the latter, one represents the maximum amount of injurious gases, and the other the minimum amount, between which limits the permissible explosives approximately vary.

Such noxious gases as may be produced by the discharge of the explosive are diluted by a much larger volume of air, and are practically harmless, as proven by actual analysis of samples taken at the face immediately after a discharge.

TABLE 1.

	Black powder.	Permissible Explosives.	
		Maximum.	Minimum.
CO_2	22.8	14.50	21.4
CO	10.3	27.74	1.3
N	10.3	45.09	74.4

In addition to investigations as to explosives for use in coal mining, the Explosives Section of the Geological Survey analyzes and tests all such materials, fuses, caps, etc., purchased by the Isthmian Canal Commission, as well as many other kinds used by the Government. It is thus acquiring a large fund of useful information, which will be published from time to time, relative to the kinds of explosives

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and the manner of using them best suited to any blasting operations, either above or under water, in hard rock, earth, or coal. There has been issued from the press, recently, a primer of explosives,* by Mr. Clarence Hall, the engineer in charge of these tests, and Professor C. E. Munroe, Consulting Explosives Chemist, which contains a large amount of valuable fundamental information, so simply expressed as to be easily understandable by coal miners, and yet sufficiently detailed to be a valuable guide to all persons who have to handle or use explosives.

In the first chapters are described the various combustible substances, and the chemical reactions leading to their explosibility. The low and high explosives are differentiated, and the sensitiveness of fulminate of mercury and other detonators is clearly pointed out. The various explosives, such as gunpowder, black blasting powder, potassium chlorate powders, nitro-glycerine powders, etc., are described, and their peculiarities and suitability for different purposes are set forth. The character and method of using the different explosives, both in opening up work and in enclosed work in coal mines, follow, with information as to the proper method of handling, transporting, storing, and thawing the same. Then follow chapters on squibs, fuses, and detonators; on methods of shooting coal off the solid; location of bore-holes; undercutting; and the relative advantages of small and large charges, with descriptions of proper methods of loading and firing the same. The subjects of explosives for blasting in rock, firing machines, blasting machines, and tests thereof, conclude the report.

The work of the chemical laboratory in which explosives are analyzed, and in which mine gases and the gases produced by combustion of explosives and explosions of coal-gas or coal dust are studied, has been of the most fundamental and important character. The Government is procuring a confidential record of the chemical composition and mode of manufacture of all explosives, fuses, etc., which are on the market. This information cannot but add greatly to the knowledge as to the chemistry of explosives for use in mines, and will furnish the basis on which remedial measures may be devised.

A bulletin (shortly to go to press) which gives the details of the physical tests of the permissible explosives thus far tested, will set

* "The Primer of Explosives," by C. E. Munroe and Clarence Hall. Bulletin No. 423, U. S. Geological Survey, 1909.

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forth elaborately the character of the testing apparatus, and the method of use and of computing results.*

This bulletin contains a chapter, by Mr. Rutledge, setting forth in detail the results of his observations as to the best methods of using permissible explosives in getting coal from various mines in which they are used. This information will be most valuable in guiding mining engineers who desire to adopt the use of permissible explosives, as to the best methods of handling them.

Electricity in Mines.—In connection with the use of electricity in mines, an informal series of tests has been made on all enclosed electric fuses, as to whether or not they will ignite an explosive mixture of air and gas when blown out. The results of this work, which is under the direction of Mr. H. H. Clark, Electrical Engineer for Mines, have been furnished the manufacturers for their guidance in perfecting safer fuses, a series of tests of which has been announced. A series of tests as to the ability of the insulation of electric wiring to withstand the attacks of acid mine waters is in progress, which will lead, it is hoped, to the development of more permanent and cheaper insulation for use in mine wiring. A series of competitive tests of enclosed motors for use in mines has been announced, and is in progress, the object being to determine whether or not sparking from such motors will cause an explosion in the presence of inflammable gas.

In the grounds outside of Building No. 10 is a large steel gallery, much shorter than Gallery No. 1, in fact, but 30 ft. in length, and much greater in diameter, namely, 10 ft. (Fig. 3, Plate X), in which electric motors, electric cutting machines, and similar apparatus, are being tested in the presence of explosive mixtures of gas and dust and with large amperage and high voltage, such as may be used in the largest electrical equipment in mines.

The investigation as to the ability of insulation to withstand the effects of acid mine waters has been very difficult and complicated. At first it was believed possible that mine waters from nearby Pennsylvania mines and of known percentages of acidity could be procured and kept in an immersion tank at approximately any given percentage of strength. This was found to be impracticable, as these waters seem to undergo rapid change the moment they are exposed to the air or are transported, in addition to the changes wrought by evap-

* "Tests of Permissible Explosives," by Clarence Hall, W. O. Snelling, S. P. Howell, and J. J. Rutledge. Bulletin No. * * *, U. S. Geological Survey.

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oration in the tank. It has been necessary, therefore, to analyze and study carefully these waters with a view to reproducing them artificially for the purpose of these tests. Concerning the insulation, delicate questions have arisen as to a standard of durability which shall be commensurate with reasonable cost. These preliminary points are being solved in conference with the manufacturers, and it is expected that the results will soon permit of starting the actual tests.

Safety-Lamp Investigations.—Many so-called safety lamps are on the market, and preliminary tests of them have been made in the lamp gallery, in Building No. 17 (Fig. 2, Plate X). After nearly a year of endeavor to calibrate this gallery, and to co-ordinate its results with those produced in similar galleries in Europe, this preliminary inquiry has been completed, and the manufacturers and agents of all safety lamps have been invited to be present at tests of their products at the Pittsburg laboratory.

A circular dated November 19th, 1909, contains an outline of these tests, which are to be conducted under the direction of Mr. J. W. Paul, an experienced coal-mining engineer and ex-Chief of the Department of State Mine Inspection of West Virginia. The lamps will be subjected to the following tests:

(*a*).—Each lamp will be placed in a mixture of air and explosive natural gas containing 6, 8, and 10% of gas, moving at a velocity of from 200 to 2 500 ft. per min., to determine the velocity of the air current which will ignite the mixture surrounding the lamp. The current will be made to move against the lamp in a horizontal, vertical ascending, and vertical descending direction, and at an angle of 45°, ascending and descending.

(*b*).—After completing the tests herein described, the lamps will be subjected to the tests described under (*a*), with the air and gas mixture under pressure up to 6 in. of water column.

(*c*).—Under the conditions outlined in (*a*), coal dust will be introduced into the current of air and gas to determine its effect, if any, in inducing the ignition of the gas mixture.

(*d*).—Each lamp will be placed in a mixture of air and varying percentages of explosive natural gas to determine the action of the gas on the flame of the lamp.

(*e*).—Each lamp will be placed in a mixture of air and varying percentages of carbonic acid gas to determine the action of the gas on the flame.

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(*f*).—Lamps equipped with internal igniters will be placed in explosive mixtures of air and gas in a quiet state and in a moving current, and the effect of the igniter on the surrounding mixture will be observed.

(*g*).—The oils (illuminants) used in the lamps will be tested as to viscosity, gravity, flashing point, congealing point, and composition.

(*h*).—Safety-lamp globes will be tested by placing each globe in position in the lamp and allowing the flame to impinge against the globe for 3 min. after the lamp has been burning with a full flame for 10 min., to determine whether the globe will break.

(*i*).—Each safety-lamp globe will be mounted in a lighted lamp with up-feed, and placed for 5 min. in an explosive mixture of air and gas moving at the rate of 1 000 ft. per min., to determine whether the heat will break the glass and, if it is broken, to note the character of the fracture.

(*j*).—Safety-lamp globes will be broken by impact, by allowing each globe to fall and strike, horizontally, on a block of seasoned white oak, the distance of fall being recorded.

(*k*).—Each safety lamp globe will be mounted in a safety lamp and, when the lamp is in a horizontal position, a steel pick weighing 100 grammes will be permitted to fall a sufficient distance to break the globe by striking its center, the distance of the fall to be recorded.

(*l*).—To determine the candle-power of safety lamps, a photometer equipped with a standardized lamp will be used. The candle-power will be determined along a line at right angles to the axis of the flame; also along lines at angles to the axis of the flame both above and below the horizontal. The candle-power will be read after the lamp has been burning 20 min.

(*m*).—The time a safety lamp will continue to burn with a full charge of illuminant will be determined.

(*n*).—Wicks in lamps must be of sufficient length to be at all times in contact with the bottom of the vessel in which the illuminant is contained, and, before it is used, the wick shall be dried to remove moisture.

Mine-Rescue Methods.—Mr. Paul, who has had perhaps as wide an experience as any mining man in the investigation of and in rescue work at mine disasters, is also in charge of the mine-rescue ap-

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paratus and training for the Geological Survey. These operations consist chiefly of a thorough test of the various artificial breathing apparatus, or so-called oxygen helmets. Most of these are of European make and find favor in Great Britain, Belgium, France, or Germany, largely according as they are of domestic design and manufacture. As yet nothing has been produced in the United States which fulfills all the requirements of a thoroughly efficient and safe breathing apparatus for use in mine disasters.

At the Pittsburg testing station there are a number of all kinds of apparatus. The tests of these are to determine ease of use, of repair, durability, safety under all conditions, period during which the supply of artificial air or oxygen can be relied on, and other essential data.

In addition to the central testing station, sub-stations for training miners, and as headquarters for field investigation as to the causes of mine disasters and for rescue work in the more dangerous coal fields, have been established; at Urbana, Ill., in charge of Mr. R. Y. Williams, Mining Engineer; at Knoxville, Tenn., in charge of Mr. J. J. Rutledge, Mining Engineer; at McAlester, Okla., in charge of Mr. L. M. Jones, Assistant Mining Engineer; and at Seattle, Wash., in charge of Mr. Hugh Wolflin, Assistant Mining Engineer. Others may soon be established in Colorado and elsewhere, in charge of skilled mining engineers who have been trained in this work at Pittsburg, and who will be assisted by trained miners. It is not to be expected that under any but extraordinary circumstances, such as those which occurred at Cherry, Ill., the few Government engineers, located at widely scattered points throughout the United States, can hope to save the lives of miners after a disaster occurs. As a rule, all who are alive in the mine on such an occasion, are killed within a few hours. This is almost invariably the case after a dust explosion, and is likely to be true after a gas explosion, although a fire such as that at Cherry, Ill., offers the greatest opportunity for subsequent successful rescue operations. The most to be hoped for from the Government engineers is that they shall train miners and be available to assist and advise State inspectors and mine owners, should their services be called for.

It should be borne in mind that the Federal Government has no

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police duties in the States, and that, therefore, its employees may not direct operations or have other responsible charge in the enforcement of State laws. There is little reason to doubt that these Federal mining engineers, both because of their preliminary education as mining engineers and their subsequent training in charge of mine operations, and more recently in mine-accidents investigations and rescue work, are eminently fitted to furnish advice and assistance on such occasions. The mere fact that, within a year, some of these men have been present at, and assisted in, rescue work or in opening up after disasters at nearly twenty of such catastrophes, whereas the average mining engineer or superintendent may be connected with but one in a lifetime, should make their advice and assistance of supreme value on such occasions. They cannot be held in any way responsible for tardiness, however, nor be unduly credited with effective measures taken after a mine disaster, because of their lack of responsible authority or charge, except in occasional instances where such may be given them by the mine owners or the State officials, from a reliance on their superior equipment for such work.

Successful rescue operations may only be looked for when the time, now believed to be not far distant, has been reached when the mine operators throughout the various fields will have their own rescue stations, as is the practice in Europe, and have available, at certain strategic mines, the necessary artificial breathing apparatus, and have in their employ skilled miners who have been trained in rescue work at the Government stations. Then, on the occurrence of a disaster, the engineer in charge of the Government station may advise by wire all those who have proper equipment or training to assemble, and it may be possible to gather, within an hour or two of a disaster, a sufficiently large corps of helmet-men to enable them to recover such persons as have not been killed before the fire—which usually is started by the explosion—has gained sufficient headway to prevent entrance into the mine. Without such apparatus, it is essential that the fans be started, and the mine cleared of gas. The usual effect of this is to give life to any incipient fire. With the apparatus, the more dense the gas, the safer the helmet-men are from a secondary explosion or from the rapid ignition of a fire, because of the absence of the oxygen necessary to combustion.

The miners who were saved at Cherry, Ill., on November 20th,

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1909, owe their lives primarily to the work of the Government engineers. The sub-station of the Survey at Urbana, Ill., was promptly notified of the disaster on the afternoon of November 13th. Arrangements were immediately made, whereby Mr. R. Y. Williams, Mining Engineer in Charge, and his Assistant, Mr. J. M. Webb, with their apparatus, were rushed by special train to the scene, arriving early the following day (Sunday).

Chief Mining Engineer, George S. Rice, Chief of Rescue Division, J. W. Paul, and Assistant Engineer, F. F. Morris, learned of the disaster through the daily press, at their homes in Pittsburg, on Sunday. They left immediately with four sets of rescue apparatus, reaching Cherry on Monday morning. Meantime, Messrs. Williams and Webb, equipped with oxygen helmets, had made two trips into the shaft, but were driven out by the heat. Both shafts were shortly resealed with a view to combating the fire, which had now made considerable headway.

The direction of the operations at Cherry, was, by right of jurisdiction, in charge of the State Mine Inspectors of Illinois, at whose solicitation the Government engineers were brought into conference as to the proper means to follow in an effort to get into the mine. The disaster was not due to an explosion of coal or gas, but was the result of a fire ignited in hay, in the stable within the mine. The flame had come through the top of the air-shaft, and had disabled the ventilating fans. A rescue corps of twelve men, unprotected by artificial breathing apparatus, had entered the mine, and all had been killed. When the shafts were resealed on Monday evening, the 15th, a small hole was left for the insertion of a water-pipe or hose. During the afternoon and evening, a sprinkler was rigged up, and, by Tuesday morning, was in successful operation, the temperature in the shaft at that time being 109° Fahr. After the temperature had been reduced to about 100°, the Federal engineers volunteered to descend into the shaft and make an exploration. The rescue party, consisting of Messrs. Rice, Paul, and Williams, equipped with artificial breathing apparatus, made an exploration near the bottom of the air-shaft and located the first body. After they had returned to the surface, three of the Illinois State Inspectors, who had previously received training by the Government engineers in the use of the rescue apparatus, including Inspectors Moses and Taylor, descended, made

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tests of the air, and found that with the fan running slowly, it was possible to work in the shaft. The rescue corps then took hose down the main shaft, having first attached it to a fire engine belonging to the Chicago Fire Department. Water was directed on the fire at the bottom of the shaft, greatly diminishing its force, and it was soon subdued sufficiently to permit the firemen to enter the mine without the protection of breathing apparatus.

Unfortunately, these operations could be pursued only under the most disadvantageous circumstances and surrounded by the greatest possible precautions, due to the frequent heavy falls of roof—a result of the heating by the mine fire—and the presence of large quantities of black-damp. All movements of unprotected rescuers had to be preceded by exploration by the trained rescue corps, who analyzed the gases, as the fire still continued to burn, and watched closely for falls, possible explosions, or a revival of the fire. While the heavy work of shoring up, and removing bodies, was being carried on by the unprotected rescue force, the helmet-men explored the more distant parts of the mine, and on Saturday afternoon, November 20th, one week after the disaster, a room was discovered in which a number of miners, with great presence of mind, had walled themselves in in order to keep out the smoke and heat. From this room 20 living men were taken, of whom 12 were recovered in a helpless condition, by the helmet-men.

This is not the first time this Government mining corps has performed valiant services. Directly and indirectly the members have saved from fifteen to twenty lives in the short time they have been organized. At the Marianna, Pa., disaster, the corps found one man still alive among 150 bodies, and he was brought to the surface. He recovered entirely after a month in the hospital.

At the Leiter mine, at Zeigler, Ill., two employees, who had been trained in the use of the oxygen helmets by members of the Government's corps, went down into the mine, following an explosion, and brought one man to the surface, where they resuscitated him.

Equally good service, either in actual rescue operations, or in explorations after mine disasters, or in fire-fighting, has been rendered by this force at the Darr, Star Junction, Hazel, Clarinda, Sewickley, Berwind-White No. 37, and Wehrum, Pa., mine disasters; at Monongah and Lick Branch, W. Va.; at Deering, Sunnyside, and Shelburn, Ind., Jobs, Ohio, and at Roslyn, Wash.

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Explosives Laboratory.—The rooms grouped at the south end of Building No. 21, at Pittsburg, are occupied as a laboratory for the chemical examination and analysis of explosives, and are in charge of Mr. W. O. Snelling.

Samples of all explosives used in the testing gallery, ballistic pendulum, pressure gauge, and other testing apparatus, are here subjected to chemical analysis in order to determine the component materials and their exact percentages. Tests are also made to determine the stability of the explosive, or its liability to decompose at various temperatures, and other properties which are of importance in showing the factors which will control the safety of the explosive during transportation and storage.

In the investigation of all explosives, the first procedure is a qualitative examination to determine what constituents are present. Owing to the large number of organic and inorganic compounds which enter into the composition of explosive mixtures, this examination must be thorough. Several hundred chemical bodies have been used in explosives at different times, and some of these materials can be separated from others with which they are mixed only by the most careful and exact methods of chemical analysis.

Following the qualitative examination, a method is selected for the separation and weighing of each of the constituents previously found to be present. These methods, of course, vary widely, according to the particular materials to be separated, it being usually necessary to devise a special method of analysis for each explosive, unless it is found, by the qualitative analysis, to be similar to some ordinary explosive, in which case the ordinary method of analysis of that explosive can be carried out. Most safety powders require special treatment, while most grades of dynamite and all ordinary forms of black blasting powder are readily analyzed by the usual methods.

The examination of black blasting powder has been greatly facilitated and, at the same time, made considerably more accurate, by means of a densimeter devised at this laboratory. In this apparatus a Torricellian vacuum is used as a means of displacing the air surrounding the grains of powder, and through very simple manipulation the true density of black powder is determined with a high degree of accuracy. In Building No. 17 there is an apparatus for separating or grading the sizes of black powder (Fig. 1, Plate X).

By means of two factors, the moisture coefficient and the hygro-

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scopic coefficient, which have been worked out at this laboratory, a number of important observations can be made on black powder, in determining the relative efficiency of the graphite coating to resist moisture, and also as a means of judging the thoroughness with which the components of the powder are mixed. The moisture coefficient relates to the amount of moisture which is taken up by the grains of the powder in a definite time under standard conditions of saturation; and the hygroscopic coefficient relates to the affinity of the constituents of the powder for moisture under the same standard conditions.

Besides the examination of explosives used at the testing station, those for the Reclamation Service, the Isthmian Canal Commission, and other divisions of the Government, are also inspected and analyzed at the explosives laboratory. At the present time, the Isthmian Canal Commission is probably the largest user of explosives in the world, and samples used in its work are inspected, tested, and analyzed at this laboratory, and at the branch laboratories at Gibbstown and Pompton Lakes, N. J., and at Xenia, Ohio.

Aside from the usual analysis of explosives for the Isthmian Canal Commission, special tests are made to determine the liability of the explosive to exude nitro-glycerine, and to deteriorate in unfavorable weather conditions. These tests are necessary, because of the warm and moist climate of the Isthmus of Panama.

Gas and Dust Gallery No. 1.—Gallery No. 1 is cylindrical in form, 100 ft. long, and has a minimum internal diameter of 6⅓ ft. It consists of fifteen similar sections, each 6⅔ ft. long and built up in in-and-out courses. The first three sections, those nearest the concrete head, are of ½-in. boiler-plate steel, the remaining twelve sections are of ⅜-in. boiler-plate steel, and have a tensile strength of, at least, 55 000 lb. per sq. in. Each section has one release pressure door, centrally placed on top, equipped with a rubber bumper to prevent its destruction when opened quickly. In use, this door may be either closed and unfastened, closed and fastened by stud-bolts, or left open. Each section is also equipped with one ¾-in. plate-glass window, 6 by 6 in., centrally placed in the side of the gallery (Fig. 1, and Figs. 1 and 2, Plate VI). The sections are held together by a lap-joint. At each lap-joint there is, on the interior of the gallery, a 2½-in. circular, angle iron, on the face of which a paper diaphragm may be placed and held in

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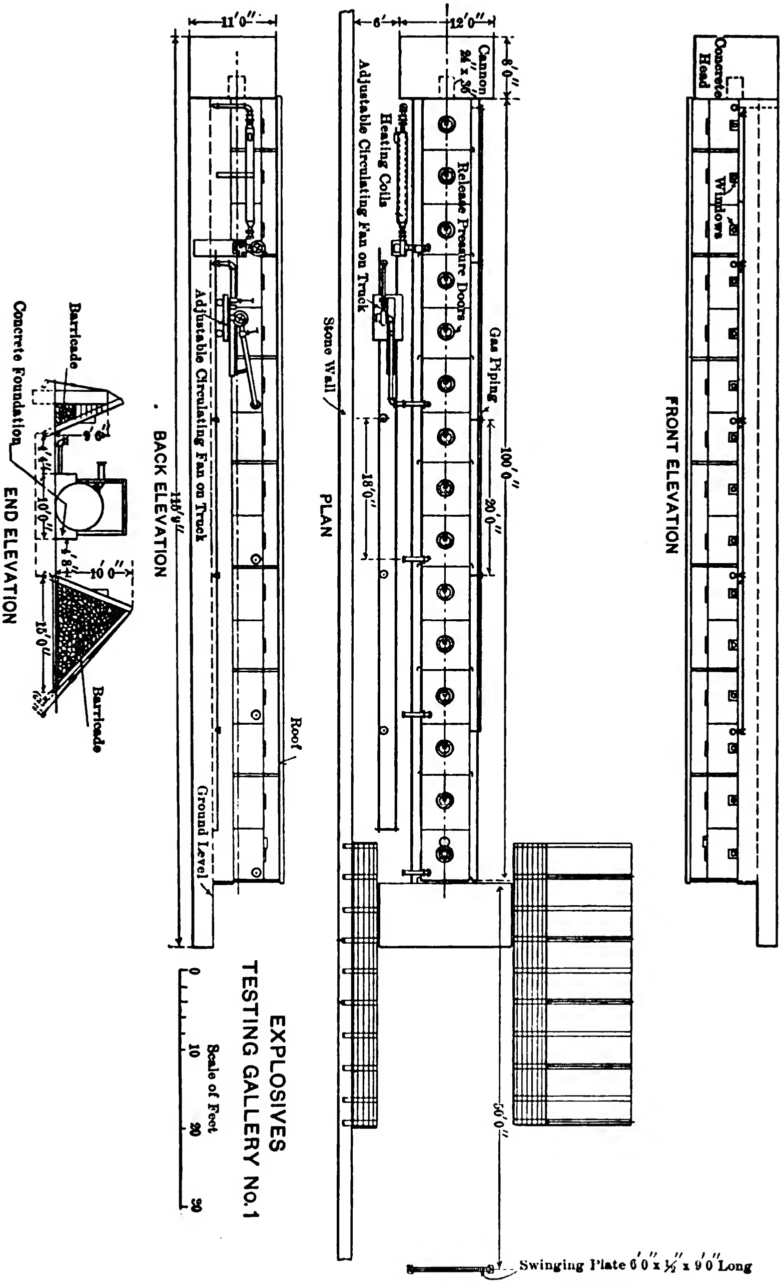
EXPLOSIVES
TESTING GALLERY No. 1
FRONT ELEVATION
Concrete Head
Windows
PLAN
Cannon 24" x 36"
Release Pressure Doors
Heating Coils
Adjustable Circulating Fan on Truck
Gas Piping
Stone Wall
100'0"
20'0"
18'0"
12'0"
6'
8'0"
50'0"
Swinging Plate 6'0" x ½" x 9'0" Long
BACK ELEVATION
11'0"
Roof
Ground Level
END ELEVATION
Barricade
Concrete Foundation
10'0"
13'0"
Scale of Feet
0
10
20
30

FIG. 1.

position by semicircular washers, studs, and wedges. These paper diaphragms are used to assist in confining a gas-and-air mixture.

Natural gas from the mains of the City of Pittsburg is used to represent that found in the mines by actual analysis. A typical analysis of this gas is as follows:

VOLUMETRIC ANALYSIS OF TYPICAL NATURAL GAS.

Hydrogen gases	0
Carbon dioxide	0.1
Oxygen	0
Heavy hydrocarbons	0
Carbon monoxide	0
Methane	81.8
Ethane	16.8
Nitrogen	1.3

The volume of gas used is measured by an accurate test meter reading to one-twentieth of a cubic foot. The required amount is admitted near the bottom, to one or more of the 20-ft. divisions of the gallery, from a 2-in. pipe, 14 ft. long. The pipe has perforations arranged so that an equal flow of gas is maintained from each unit length.

Each 20-ft. division of the gallery is further equipped with an exterior circulating system, as shown by Fig. 1, thus providing an efficient method of mixing the gas with the air. For the first division this circulating system is stationary, a portion of the piping being equipped with heating coils for maintaining a constant temperature.

The other divisions have a common circulating system mounted on a truck which may be used on any of these divisions. Valves are provided for isolating the fan so that a possible explosion will not injure it.

In the center section of each division is an indicator cock which is used to provide means of recording pressures above and below atmospheric, or of sampling the air-and-gas mixture. The first division of the gallery is equipped with shelves laterally placed, for the support of coal dust.

The cannon in which the explosive is fired is placed in the concrete head, the axial line of the bore-hole being coincident with that of the gallery. This cannon (Fig. 2) is similar to that used in the ballistic pendulum. The charge is fired electrically from the observation room. To minimize the risk of loading the cannon, the charger carries in his

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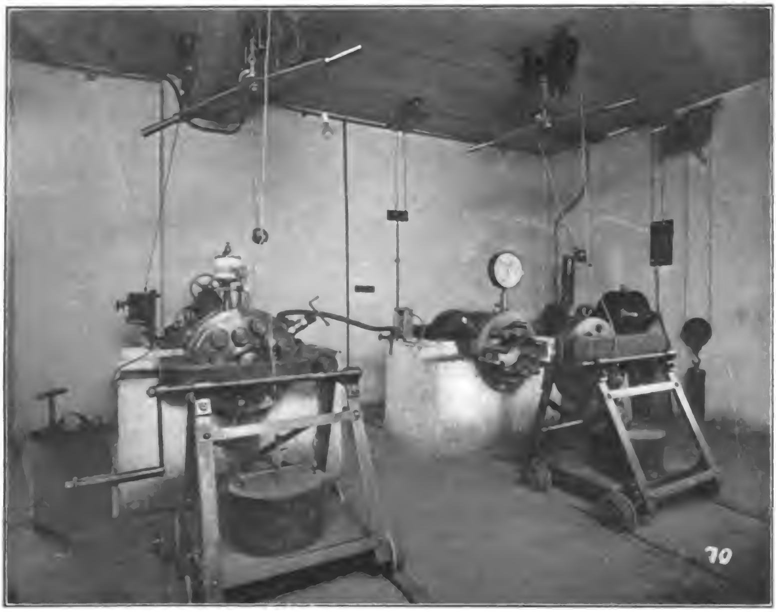

FIG. 1.—BICHEL PRESSURE GAUGES.

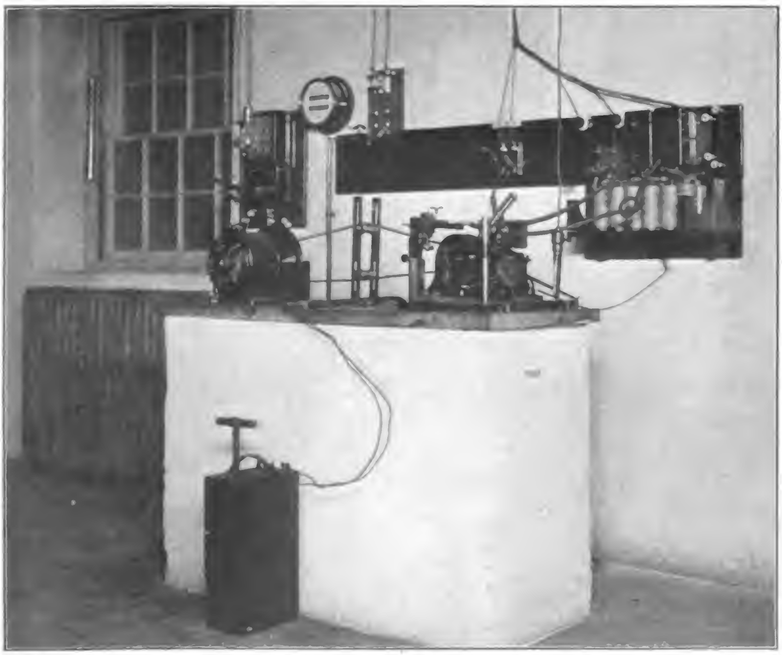

FIG. 2.—RATE OF DETONATION RECORDER.

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pocket the plug of a stage switch (the only plug of its kind on the ground), so that it is impossible to complete the circuit until the charger has left the gallery. That portion of the first division of the gallery which is not embedded in concrete, has a 3-in. covering made up of blocks of magnesia, asbestos fiber, asbestos, cement, a thin layer of 8-oz. duck, and strips of water-proof roofing paper, the whole being covered with a thick coat of graphite paint. The object of this covering is to assist in maintaining a constant temperature.

The entire gallery rests on a concrete foundation 10 ft. wide, which has a maximum height of 4½ ft. and a minimum height of 2 ft.

The concrete head in which the cannon is placed completely closes that end of the gallery. A narrow drain extends under the entire length of the gallery, and a tapped hole at the bottom of each section provides an efficient means of drainage.

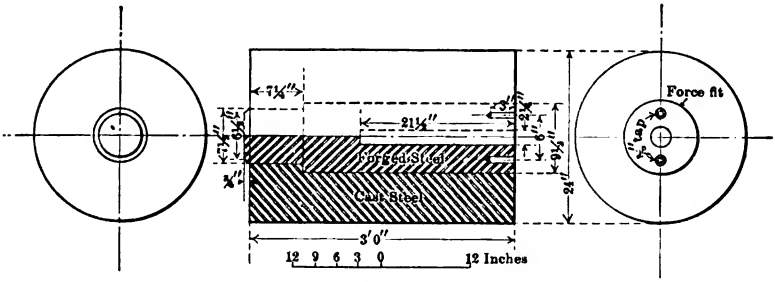

FIG. 2.

The buildings near the gallery are protected by two barricades near the open end, each 10 ft. high and 30 ft. long. A back-stop, consisting of a swinging steel plate, 6 ft. high and 9 ft. long, 50 ft. from the end of the gallery, prevents any of the stemming from doing damage.

Tests are witnessed from an observation room, a protected position about 60 ft. from the gallery. The walls of the room are 18 in. thick, and the line of vision passes through a ½-in. plate glass, 6 in. wide and 37 ft. long, and is further confined by two external guards, each 37 ft. long and 3 ft. wide.

In this gallery a series of experiments has been undertaken to determine the amount of moisture necessary with different coal dusts, in order to reduce the likelihood of a coal-dust explosion from a blown-out shot of one of the dangerous types of explosives.

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Coal dust taken from the roads of one of the coal mines in the Pittsburg district required at least 12% of water to prevent an ignition. It has also been proven that the finer the dust the more water is required, and when it was 100-mesh fine, 30% of water was required to prevent its ignition by the flame of a blown-out shot in direct contact. The methods now used in sprinkling have been proven entirely insufficient for thoroughly moistening the dust, and hence are unreliable in preventing a general dust explosion.

At this station successful experiments have been carried out by using humidifiers to moisten the atmosphere after the temperature of the air outside the gallery has been raised to mine temperature and drawn through the humidifiers. It has been found that if a relative humidity of 90%, at a temperature of 60° Fahr., is maintained for 48 hours, simulating summer conditions in a mine, the absorption of moisture by the dust and the blanketing effect of the humid air prevent the general ignition of the dust.

These humidity tests have been run in Gas and Dust Gallery No. 1 with special equipment consisting of a Koerting exhauster having a capacity of 240 000 cu. ft. per hour, which draws the air out of the gallery through the first doorway, or that next the concrete head in which the cannon is embedded.

The other end of the gallery is closed by means of brattice cloth and paper diaphragms, the entire gallery being made practically airtight. The air enters the fifteenth doorway through a box, passing over steam radiators to increase its temperature, and then through the humidifier heads.

Explosives Testing Apparatus.

There is no exposed woodwork in Building No. 17, which is 40 by 60 ft., two stories high, and substantially constructed of heavy stone masonry, with a slate roof. The structure within is entirely fire-proof. Iron columns and girders, and wooden girders heavily encased in cement, support the floors which are either of cement slab construction or of wooden flooring protected by expanded metal and cement mortar, both above and beneath. At one end, on the ground floor, is the exposing and recording apparatus for flame tests of explosives, also pressure gauges, and a calorimeter, and, at the other end, is a gallery for testing safety lamps.

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The larger portion of the second floor is occupied by a gas-tight training room for rescue work, and an audience chamber, from which persons interested in such work may observe the methods of procedure. A storage room for rescue apparatus and different models of safety lamps is also on this floor.

The disruptive force of explosives is determined in three ways, namely, by the ballistic pendulum, by the Bichel pressure gauge, and by Trauzl lead blocks.

Ballistic Pendulum.—The disruptive force of explosives, as tested by the ballistic pendulum, is measured by the amount of oscillation. The standard unit of comparison is a charge of ½ lb. of 40% nitroglycerine dynamite. The apparatus consists essentially of a 12-in. mortar (Fig. 3, Plate VI), weighing 31 600 lb., and suspended as a pendulum from a beam having knife-edges. A steel cannon is mounted on a truck set on a track laid in line with the direction of the swing of the mortar. At the time of firing the cannon may be placed $\frac{1}{16}$-in. from the muzzle of the mortar. The beam, from which the mortar is suspended, rests on concrete walls, 51 by 120 in. at the base and 139 in. high. On top of each wall is a 1-in. base-plate, 7 by 48 in., anchored to the wall by ⅝-in. bolts, 28 in. long. The knife-edges rest on bearing-plates placed on these base-plates. The bearing-plates are provided with small grooves for the purpose of keeping the knife-edges in oil and protected from the weather. The knife-edges are each 6 in. long, $2\frac{11}{16}$ in. deep from point to back, 2 in. wide at the back, and taper 50° with the horizontal, starting on a line 1½ in. from the back. The point is rounded to conform to a radius of ¼ in. The back of each is 2 in. longer than the edge, making a total length of 10 in., and is 1 in. deep and 12 in. wide. This shoulder gives bolting surface to the beam from which the mortar is hung. The beam is of solid steel, has a 4 by 8-in. section, and is 87 in. long. Heavy steel castings are bolted to it to take the threads of the machine-steel rods which form the saddles on which the mortar is suspended. The radius of the swing, measured from the point of the knife-edges to the center of the trunnions, is 89¾ in.

The cannon consists of two parts, a jacket and a liner. The jacket is 36 in. long, has an external diameter of 24 in., and internal diameters of 9½ and 7½ in. It is made of the best cast steel or of forged steel.

The liner is 36½ in. long, with a 1-in. shoulder, 7¾ in. from the

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back, changing the diameter from 9½ to 7½ in. The bore is smooth, being 2¼ in. in diameter and 21½ in. long. The cannon rests on a 4-wheel truck, to which it is well braced by straps and rods. A track of 30-in. gauge extends about 9 ft. from the muzzle of the mortar to the bumper for the cannon.

The shot is fired by an electric firing battery, from the first floor of Building No. 17, about 10 yd. away. To insure the safety of the operator and the charger, the man who loads the cannon carries a safety plug without which the charge cannot be exploded. The wires for connecting to the fuse after charging are placed conveniently, and the safety plug is then inserted in a box at the end of the west wall. The completion of the firing battery by the switch at the firing place is indicated by the flashing of a red light, after which all that is necessary to set off the charge is to press a button on the battery. An automatic recording device at the back of the mortar records the length of swing which, by a vernier, may be read to $\frac{1}{100}$ in.

Bichel Pressure Gauges.—Pressure gauges are constructed for the purpose of determining the unit disruptive force of explosives detonating at different rates of velocity, by measuring pressures developed in an enclosed space from which the generated gases cannot escape. The apparatus consists of a stout steel cylinder, which may be made absolutely air-tight; an air-pump and proper connections for exhausting the air in the cylinder to a pressure equivalent to 10 mm. of mercury; an insulated plug for providing the means of igniting the charge; a valve by which the gaseous products of combustion may be removed for subsequent analysis; and an indicator drum (Fig. 1, Plate VII) with proper connections for driving it at a determinable speed.

This apparatus is in the southeast corner of Building No. 17. The cylinder is 31½ in. long, 19¾ in. in diameter, and is anchored to a solid concrete footing at a convenient height for handling. The explosion chamber is 19 in. long and 7¾ in. in diameter, with a capacity of exactly 15 liters. The cover of the cylinder is a heavy piece of steel held in place by stout screw-bolts and a heavy steel clamp.

The charge is placed on a small wire tripod, and connections are made with a fuse to an electric firing battery for igniting the charges. The cover is drawn tight, with the twelve heavy bolts against lead washers. The air in the cylinder is exhausted to 10 mm., mercury column, in order to approach more closely the conditions of a stemmed

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FIG. 1.—EXPLOSIVES CALORIMETER.

FIG. 2.—BUILDING NO. 17, AND FLAME-TEST APPARATUS.

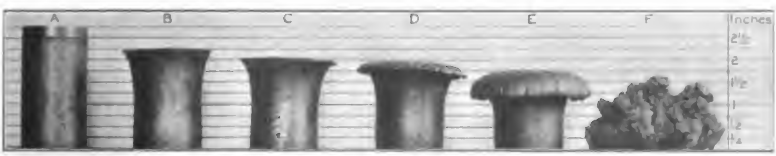

FIG. 3.—SMALL LEAD BLOCK TEST.

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charge exploding in a bore-hole inaccessible to air; the indicator drum is placed in position and set in motion; and, finally, the shot is fired. The record shown on the indicator card is a rapidly ascending curve for quick explosives and a shallower, slowly rising curve for explosives of slow detonation. When the gases cool, the curve merges into a straight line, which indicates the pressures of the cooled gases on the sides of the chamber.

Since the ratio of the volume of the cylinder to the volume of the charge may be computed, the pressure of the confined charge may also be found, and this pressure often exceeds 100 000 lb. per sq. in. The cooling effect of the inner surface on the gaseous products of combustion, a vital point in computations of the disruptive force of explosives by this method, is determined by comparing the pressures obtained in the original cylinder with those in a second cylinder of larger capacity, into which has been inserted one or more steel cylinders to increase the superficial area while keeping the volume equal to that of the first cylinders. By comparing results, a curve may be plotted, which will determine the actual pressures developed, with the surface-cooling effect eliminated.

Trauzl Lead Blocks.—The lead-block test is the method adopted by the Fifth International Congress of Applied Chemistry as the standard for measuring the disruptive force of explosives. The unit by this test is defined to be the force required to enlarge the bore-hole in the block to an amount equivalent to that produced by 10 grammes of standard 40% nitro-glycerine dynamite stemmed with 50 grammes of dry sand under standard conditions as produced with the tamping device. The results of this test, when compared with those of the Bichel gauge, indicate that, for explosives of high detonation, the lead block is quite accurate, but for slow explosives, such as gunpowder, the expansion of the gases is not fast enough to make comparative results of value. The reason for this is that the gases escape through the bore of the block rather than take effect in expanding the bore-hole.

The lead blocks are cylindrical, 200 mm. in diameter, and 200 mm. high. Each has a central cavity, 25 mm. in diameter and 125 mm. deep (Fig. 1, Plate IX), in which the charge is placed. The blocks are made of desilverized lead of the best quality, and, as nearly as possible, under identical conditions. The charge is placed in the cavity and prepared for detonation with an electrical exploder and

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stemming. After the explosion the bore-hole is pear-shaped, the size of the cavity depending, not only on the disruptive power of the explosive, but also on its rate of detonation, as already indicated. The size of the bore-hole is measured by filling the cavity with water from a burette. The difference in the capacity of the cavity before and after detonation indicates the enlarging power of the explosive.

Calorimeter.—The explosion calorimeter is designed to measure the amount of heat given off by the detonation of explosive charges of 100 grammes. The apparatus consists of the calorimeter bomb (Fig. 1, Plate VIII), the inner receiver or immersion vessel, a wooden tub, a registering thermometer, and a rocking frame. This piece of apparatus stands on the east side of Building No. 17.

The bottle-shaped bomb is made of ½-in. wrought steel, and has a capacity of 30 liters. On opposite sides near the top are bored apertures, one for the exhaust valve for obtaining a partial vacuum (about 20 mm., mercury column) after the bomb has been charged, the other for inserting the plug through which passes the fuse wire for igniting the charge. The bomb is closed with a cap, by which the chamber may be made absolutely air-tight. It is 30 in. high with the cap on, weighs 158 lb., and is handled to and from the immersion vessel by a small crane.

The inner receiver is made of $\frac{1}{16}$-in. sheet copper, 30⅞ in. deep, and with an inner diameter of 17⅞ in. It is nickel-plated, and strengthened on the outside with bands of copper wire, and its capacity is about 70 liters. The outer tub is made of 1-in. lumber strengthened with four brass hoops on the outside. It is 33 in. deep, and its inner diameter is 21 in.

The stirring device, operated vertically by an electric motor, consists of a small wooden beam connected to a system of three rings having a horizontal bearing surface. When the apparatus is put together, the inner receiver rests on a small standard on top of the base of the outer tank, and the rings of the stirring device are run between the bomb and the inner receiver. The bomb itself rests on a small standard placed on the bottom of the inner receiver. The apparatus is provided with a snugly fitting board cover. The bomb is charged from the top, the explosive being suspended in its center. The air is exhausted to the desired degree of rarification. The caps are then screwed on, and the apparatus is set together as described.

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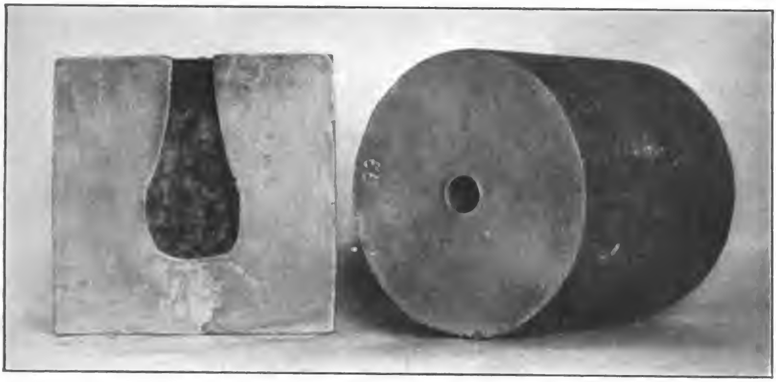

FIG. 1.—TRAUZL LEAD BLOCKS.

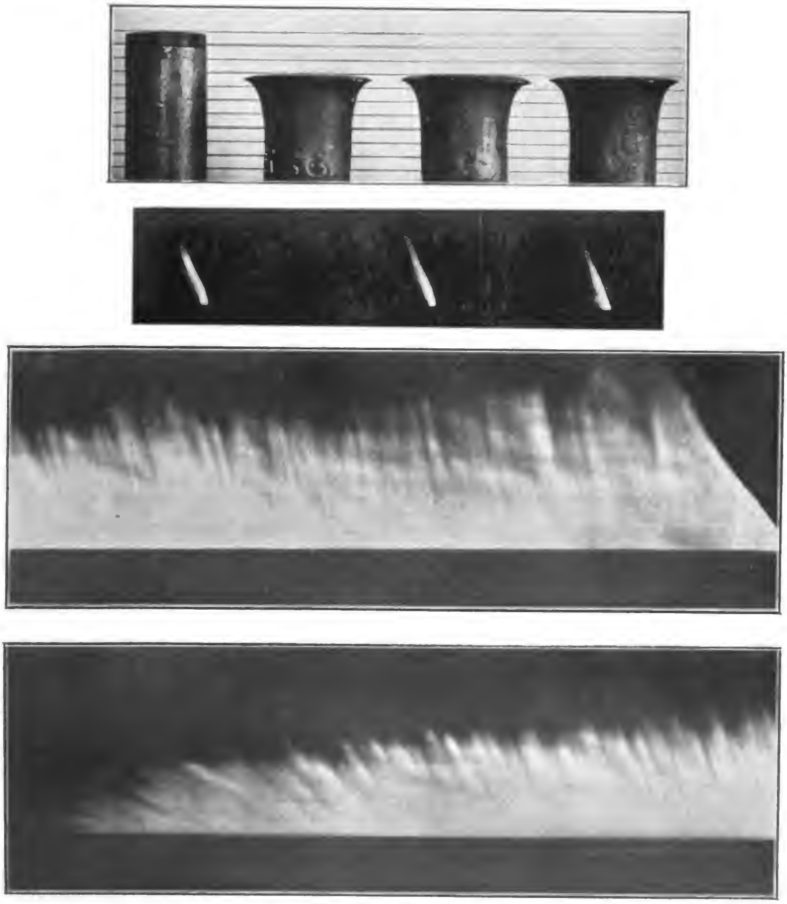

FIG. 2.—POWDER FLAMES.

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The apparatus is assembled on scales and weighed before the water is poured in and after the receiver is filled. From the weight of the water thus obtained and the rise of temperature, the calorific value may be computed. The charge is exploded by electricity, while the water is being stirred. The rise in the temperature of the water is read by a magnifying glass, from a thermometer which measures temperature differences of 0.01 degree. From the readings obtained, the maximum temperature of explosion may be determined, according to certain formulas for calorimetric experiments. Proper corrections are made for the effects, on the temperature readings, of the formation of the products of combustion, and for the heat-absorbing power of the apparatus.

Impact Machine.—In Building No. 17, at the south side, is an impact machine designed to gauge the sensitiveness of explosives to shock. For this purpose, a drop-hammer, constructed to meet the following requirements, is used: A substantial, unyielding foundation; minimum friction in the guide-grooves; and no escape or scattering of the explosive when struck by the falling weight. This machine is modeled after one used in Germany, but is much improved in details of construction.

The apparatus, Fig. 1, Plate XI, consists essentially of the following parts: An endless chain working in a vertical path and provided with lugs; a steel anvil on which the charge of explosive is held by a steel stamp; a demagnetizing collar moving freely in vertical guides and provided with jaws placed so that the lugs of the chain may engage them; a steel weight sliding loosely in vertical guides and drawn by the demagnetizing collar to determinable heights when the machine is in operation; a second demagnetizing collar, which may be set at known heights, and provided with a release for the jaws of the first collar; and a recording device geared to a vertically-driven threaded rod which raises or lowers, sets the second demagnetizing collar, and thus determines the height of fall of the weight. By this apparatus the weight may be lifted to different known heights, and dropped on the steel stamp which transmits the shock to the explosive. The fall necessary to explode the sample is thus determined.

The hammers are of varying weight, the one generally used weighing 2 000 grammes. As the sensitiveness of an explosive is influenced by temperature changes, water at 25° cent. is allowed to flow through the anvil in order to keep its temperature uniform.

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Flame Test.—An apparatus, Fig. 2, Plate VIII, designed to measure the length and duration of flames given off by explosives, is placed at the northeast corner of Building No. 17. It consists essentially of a cannon, a photographing device, and a drum geared for high speed, to which a sensitized film may be attached.

About 13 ft. outside the wall of Building No. 17, set in a concrete footing, is a cannon pointing vertically into an encasing cylinder or stack, 20 ft. high and 43 in. in diameter. This cannon is a duplicate of the one used for the ballistic pendulum, details of which have already been given. The stack or cylinder is of ¼-in. boiler plate, in twenty-four sections, and is absolutely tight against light at the base and on the sides. It is connected with a dark room in Building No. 17 by a light-tight conduit of rectangular section, 12 in. wide, horizontal on the bottom, and sloping on the top from a height of 8¼ ft. at the stack to 21 in. at the inside of the wall of the building.

The conduit is carefully insulated from the light at all joints, and is riveted to the stack. A vertical slit, 2 in. wide and 8 ft. long, coincident with the center line of the conduit, is cut in the stack. A vertical plane drawn through the center line of the bore-hole of the cannon and that of the slit, if produced, intersects the center line of a quartz lens, and coincides with the center of a stenopaic slit and the axis of the revolving drum carrying the film. The photographing apparatus consists of a shutter, a quartz lens, and a stenopaic slit, 76 by 1.7 mm., between the lens and the sensitized film on the rotary drum. The quartz lens is used because it will focus the ultra-violet rays, which are those attending extreme heat.

The drum is 50 cm. in circumference and 10 cm. deep. It is driven by a 220-volt motor connected to a tachometer which reads both meters per second and revolutions per minute. A maximum peripheral speed of 20 m. per sec. may be obtained.

When the cannon is charged, the operator retires to the dark room in which the recording apparatus is located, starts the drum, obtains the desired speed, and fires the shot by means of a battery. When developed, the film shows a blur of certain dimensions, produced by the flame from the charge. From the two dimensions—height and lateral displacement—the length and duration of the flame of the explosive are determined.

The results of flame tests of a permissible explosive and a test of black blasting powder, all shot without stemming, are shown on

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Fig. 2, Plate IX. In this test, the speed of the drum carrying the black powder negative was reduced to one sixty-fourth of that for the permissible explosives, in order that the photograph might come within the limits of the negative. In other words, the duration of the black powder flame, as shown, should be multiplied by 64 for comparison with that of the permissible explosive, which is from 3 500 to 4 000 times quicker.

Apparatus for Measuring Rate of Detonation.—The rate at which detonation travels through a given length of an explosive can be measured by an apparatus installed in and near Building No. 17. Its most essential feature is a recording device, with an electrical connection, by which very small time intervals can be measured with great exactness.

The explosive is placed in a sheet-iron tube about 1½ in. in diameter and 4 ft. long, and suspended by cords in a pit, 11 ft. deep and 16 ft. in diameter. This pit was once used as the well of a gas tank, Fig. 2, Plate VIII. In adapting the pit to its new use, the tank was cut in two; the top half, inverted, was placed in the pit on a bed of saw-dust, and the space between the tank and the masonry walls of the pit was filled with saw-dust. The cover of the pit consists of heavy timbers framed together and overlaid by a 12-in. layer of concrete reinforced by six I-beams. Four straps extend over the top and down to eight "deadmen" planted about 8 ft. below the surface of the ground.

The recording device, known as the Mettegang recorder, Fig. 2, Plate VII, comprises two sparking induction coils and a rapidly revolving metallic drum driven by a small motor, the periphery of the drum having a thin coating of lampblack. A vibration tachometer which will indicate any speed between 50 and 150 rev. per sec., is directly connected to the drum, so that any chance of error by slipping is eliminated. The wires leading to the primary coils of the sparking coils pass through the explosive a meter or more apart. Wires lead from the secondary coils to two platinum points placed a fraction of a millimeter from the periphery of the drum. A separate circuit is provided for the firing lines.

In making a test, the separate cartridges, with the paper trimmed from the ends, are placed, end to end, in the sheet-iron tube; the drum is given the desired peripheral speed, and the charge is exploded. The usual length between the points in the tube is 1 m., and the time required for the detonation of a charge of that length is shown by the

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distance between the beginning of two rows of dots on the drum made by the sparks from the secondary coil circuits, the dots starting the instant the primary circuits are broken by the detonation. At one end of the drum are gear teeth, 1 mm. apart on centers, which can be made to engage a worm revolving a pointer in front of a dial graduated to hundredths; by means of this and a filar eyepiece, the distance between the start of the two rows of spark dots on the drum can be measured accurately to 0.01 mm. As the drum is 500 mm. in circumference, and its normal speed is 86 rev. per sec., it is theoretically possible to measure time to one four-millionth of a second, though with a cartridge 1 m. long, such refinement has not been found necessary.

The use of small lead blocks affords another means of determining the rate of detonation or quickness of an explosive. Each block (a cylinder, 2½ in. long and 1½ in. in diameter) is enclosed in a piece of paper so that a shell is formed above the block, in which to place the charge. A small steel disk of the same diameter as the block is first placed in the shell on top of the block, then the charge with a detonator is inserted. The charge is customarily 100 grammes. On detonation of the charge, a deformation of the lead takes place, the amount of which is due to the quickness of the explosive used (Fig. 3, Plate VIII).

Sample Record of Tests.

The procedure followed in the examination of an explosive is shown by the following outline:

1.—Physical Examination.

(*a*).—Record of appearance and marks on original package.

(*b*).—Dimensions of cartridge.

(*c*).—Weight of cartridge, color and specific gravity of powder.

2.—Chemical Analysis.

(*a*).—Record of moisture, nitro-glycerine, sodium or potassium nitrate, and other chemical constituents, as set forth by the analysis; percentage of ash, hygroscopic coefficient—the amount of water taken up in 24 hours in a saturated atmosphere, at 15° cent., by 5 grammes, as compared with the weight of the explosive.

(*b*).—Analysis of products of combustion from 100 grammes, including gaseous products, solids, and water.

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(*c*).—Composition of gaseous products of combustion, including carbon monoxide and carbon dioxide, hydrogen, nitrogen, etc.

(*d*).—Composition of solid products of combustion, subdivided into soluble and insoluble.

3.—A Typical Analysis of Natural Gas.

Used in tests, as follows:

Carbon dioxide	0.0 per cent.
Heavy hydrocarbons	0.2 " "
Oxygen	0.1 " "
Carbon monoxide	0.0 " "
Methane	82.4 " "
Ethane	15.3 " "
Nitrogen	2.0 " "
	100.00 per cent.

4.—Typical Analysis of Bituminous Coal Dust, 100-Mesh Fine, Used in Tests.

Moisture	1.90
Volatile matter	35.05
Fixed carbon	58.92
Ash	4.13
	100.00
Sulphur	1.04

5.—An Average Analysis of Detonators.

Used on Trauzl lead blocks, pressure gauge, calorimeter, and small lead blocks:

$M - l\frac{l}{m}$. Triple-strength exploder.

Charge ....1.5729 grammes.

	Mercury fulminate.	Chlorate of potash.
Specification	89.73	10.27

Used on all other tests:

$M - 260\frac{l}{m}$. Double-strength exploder.

Charge ....0.9805 grammes.

	Mercury fulminate.	Chlorate of potash.
Specification	91.31	8.69

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6.—Ballistic-Pendulum Tests.

This record includes powder used, weight of charge, swing of mortar, and unit disruptive charge, the latter being the charge required to produce a swing of the mortar equal to that produced by ½ lb. (227 grammes) of 40% dynamite, or 3.01 in.

7.—Record of Tests.

Tests Nos. 1 to 5 in Gallery No. 1, as set forth in preceding circular.

8.—Trauzl Lead-Block Test.

Powder and test numbers, expansion of bore-hole in cubic centimeters, and average expansion compared with that produced by a like quantity (10 grammes) of 40% dynamite, the latter giving an average expansion of 294 cu. cm.

9.—Pressure Gauge.

Powder and test number, weight of charge, charging density, height of curve, pressure developed, and pressure developed after cooling, compared with pressure developed after elimination of surface influences by a like quantity (100 grammes) of 40% dynamite, the average being 8 439 kg. per sq. cm.

10.—Rate of Detonation.

Powder and test number, size of cartridge, and rate of detonation in meters per second, for comparison with rate of detonation of 40% dynamite, which, under the same conditions, averages 4 690 m. per sec.

11.—Impact Machine.

Explosive and test numbers, distance of fall (2 000-gramme weight) necessary to cause explosion, for comparison with length of fall, 11 cm., necessary to cause explosion of 40% dynamite.

12.—Distance of Explosive Wave Transmitted by 1.25 by 8-in. Cartridge.

Explosive and test numbers, weight of cartridge, distance separating cartridges in tests, resulting explosion or non-explosion, for comparison with two cartridges of 40% dynamite, hung, under identical conditions, 13 in. apart, end to end, in which case detonation of the first cartridge will explode the second.

13.—Flame Test.

Explosive and test numbers, charge 100 grammes with 1 lb. of clay stemming, average length of flame and average duration of flame, for comparison with photographs produced by 40% dynamite under like conditions.

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FIG. 1.—SEPARATOR FOR GRADING BLACK POWDER.

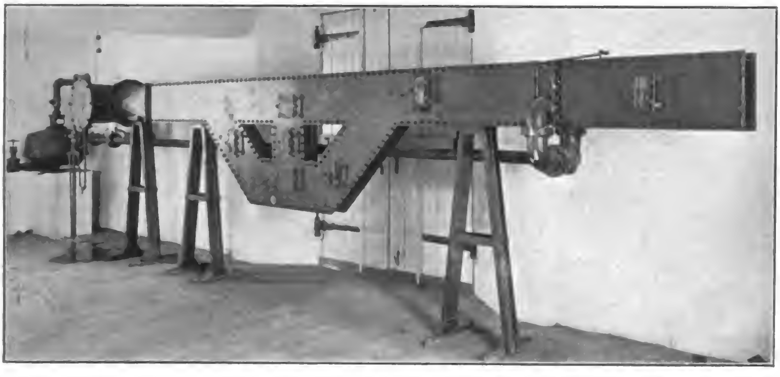

FIG. 2.—SAFETY LAMP TESTING GALLERY.

FIG. 3.—MINE GALLERY NO. 2.

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14.—Small Lead Blocks.

Powder and test numbers, weight of charge, and compression produced in blocks.

15.—Calories Developed.

Number of large calories developed per kilogramme of explosive, for comparison with 1 000 grammes of 40% dynamite, which develop, on an average, 1 229 large calories.

Blasting Powder Separator.

The grains of black blasting powder are graded by a separator, similar to those used in powder mills, but of reduced size. It consists of an inclined wooden box, with slots on the sides to carry a series of screens, and a vertical conduit at the end for carrying off the grains as they are screened into separate small bins (Fig. 1, Plate X). At the upper end of the screens is a small 12 by 16-in. hopper, with a sliding brass apron to regulate the feed. The screens are shaken laterally by an eccentric rod operated by hand. The top of the hopper is about 6½ ft. above the floor. The box is 6 ft. 10 in. long, from tip to tip, and inclines at an angle of 9 degrees.

After separation the grains fall through a vertical conduit, and thence to the bins through zinc chutes, 1 by 2 in. in section. Care is taken to have no steel or iron exposed to the powder.

The screens are held by light wooden frames which slip into the inclined box from the upper end. In this way, any or all of the screens may be used at once, thus separating all grades, or making only such separations as are desired. The screens with the largest meshes are diagonally-perforated zinc plates. Table 2 gives the number of holes per square foot in zinc plates perforated with circular holes of the diameters stated.

TABLE 2.—NUMBER OF HOLES PER SQUARE FOOT IN ZINC PLATES WITH CIRCULAR PERFORATIONS.

Diameter, in inches.	Number of holes.	Diameter, in inches.	Number of holes.	Diameter, in inches.	Number of holes.	Diameter, in inches.	Number of holes.
$\frac{1}{2}$	853	$\frac{3}{8}$	782	$\frac{1}{5}$	1 680	$\frac{1}{10}$	6 636
$\frac{7}{16}$	518	$\frac{1}{4}$	1 892	$\frac{1}{8}$	3 456	$\frac{1}{16}$	12 900

The finer meshes are obtained by using linen screens with holes of two sizes, namely, $\frac{1}{20}$ in. square and $\frac{1}{28}$ in. square.

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Until a few years ago, black blasting powder was manufactured in the sizes given in Table 3.

TABLE 3.—GRADATION OF BLACK BLASTING POWDER.

Grade.	Mesh.	Grade.	Mesh.
CC	2-2½	FF	5- 8
C	2½-3	FFF	8-16
F	3-5	FFFF	16-28

In late years there has been considerable demand for special sizes and mixed grains for individual mines, especially in Illinois. As no material change has been made in the brands, the letters now used are not indicative of the size of the grains, which they are supposed to represent. Of 29 samples of black blasting powder recently received from the Illinois Powder Commission, only 10 were found to contain 95% of the size of grains they were supposed to represent; 4 contained 90%; 7 varied from 80 to 90%; several others were mixtures of small and large grains, and were branded FF black blasting powder; and one sample contained only 8.5% of the size of grains it was supposed to represent. The remaining samples showed many variations, even when sold under the same name. The practice of thus mixing grades is exceedingly dangerous, because a miner, after becoming accustomed to one brand of FF powder of uniform separation, may receive another make of similar brand but of mixed grains, and, consequently, he cannot gauge the quantity of powder to be used. The result is often an over-load or a blown-out shot. The smaller grains will burn first, and the larger ones may be thrown out before combustion is complete, and thus ignite any fire-damp present.

Lamp Testing Gallery.

At the Pittsburg testing station, there is a gallery for testing safety lamps in the presence of various percentages of inflammable gas. In this gallery the safety of the lamps in these gaseous mixtures may be tested, and it is also possible for mine inspectors and fire bosses to bring their safety lamps to this station, and test their measurements of percentage of gas, by noting the length and the appearance of the flame in the presence of mixtures containing known percentages of methane and air.

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PLATE XI.
TRANS. AM. SOC. CIV. ENGRS.
VOL. LXX, No. 1171.
WILSON ON
FEDERAL INVESTIGATIONS OF MINE ACCIDENTS, ETC.

FIG. 1.—IMPACT MACHINE.

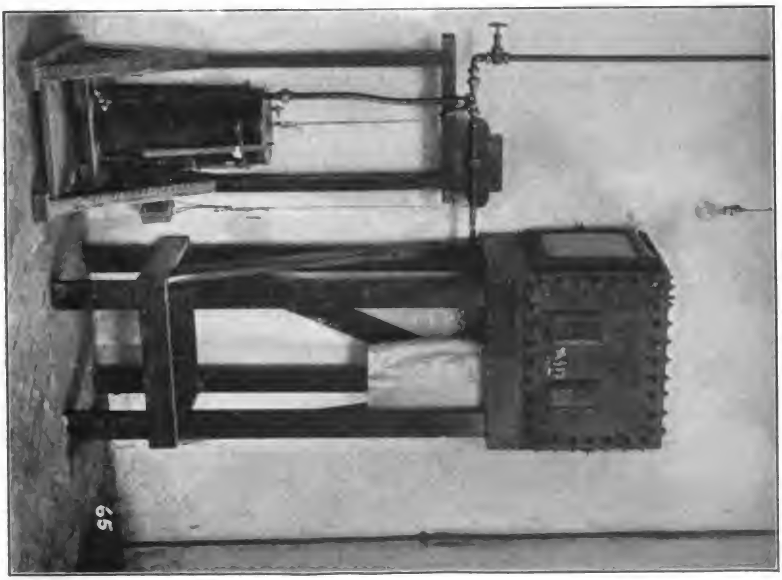

FIG. 2.—LAMP TESTING BOX.

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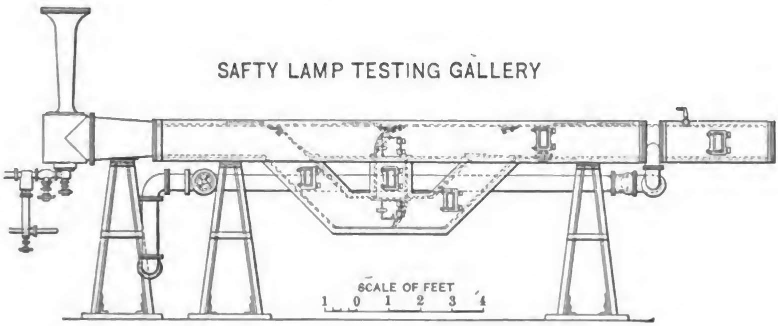
The gas-tight gallery used for testing the lamps, consists of a rectangular conduit (Fig. 2, Plate X), having sheet-steel sides, 6 mm. thick and 433 mm. wide, the top and bottom being of channel iron. The gallery rests on two steel trestles, and to one end is attached a No. 5 Koerting exhauster, capable of aspirating 50 cu. m. per min., under a pressure of 500 mm. of water, with the necessary valve, steam separator, etc. The mouth of the exhauster passes through the wall of the building and discharges into the open air.

Besides the main horizontal conduit, there are two secondary conduits connected by a short horizontal length, and the whole is put together so that the safety lamp under test may be placed in a current of air, or of air and gas, which strikes it horizontally, vertically upward or downward, or at an angle of 45° (Fig. 3). The path of the current is determined by detachable sheet-steel doors.



ELEVATION

Fig. 3.

There are five double observing windows of plate glass, which open on hinges. The size of each window is 7½ by 3 in.; the inner glass is ¼ in. thick and the outer one, ½ in. thick. These glasses are separated by a space of ¼ in. The upper conduit has four safety doors along the top, each of the inclined conduits has one safety door, and the walls and windows are provided with rubber gaskets or asbestos packing, to make them gas-tight. The cross-sectional area of the conduit is 434 sq. cm.

The air inlet consists of 36 perforations, 22 mm. in diameter, in a bronze plate or diaphragm. The object of this diaphragm is to produce pressure in the conduit before the mixing boxes, and permit the measuring of the velocity of the current. The air-current, after

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passing through the holes, enters the mixer, a cast-steel box traversed by 36 copper tubes, each perforated by 12 openings, 3 mm. in diameter, arranged in a spiral along its length and equally spaced. The total cross-sectional area of the tubes is 137 sq. cm.

The explosive gas enters the interior of the box around the tubes through large pipes, each 90 mm. in diameter, passes thence through the 432 openings in the copper tubes, and mixes thoroughly with the air flowing through these tubes. The current through the apparatus is induced by the exhauster, and its course is determined by the position of the doors.

The gallery can be controlled so as to provide rapidly and easily a current of known velocity and known percentage of methane. In the explosive current of gas and air, safety lamps of any size or design can be tested under conditions simulating those found occasionally in mines, air-currents containing methane in dangerous proportions striking the lamps at different angles, and the relative safety of the various types of lamps under such conditions can be determined. In this gallery it is also possible to test lighting devices either in a quiet atmosphere or in a moving current, and, by subjecting the lamps to air containing known percentages of methane, it is possible to acquaint the user with the appearance of the flame caps.

Breathing Apparatus.

With this apparatus, the wearer may explore a gaseous mine, approach fires for the purpose of fighting them, or make investigations after an explosion. Its object is to provide air or oxygen to be breathed by the wearer in coal mines, when the mine air is so full of poisonous gases as to render life in its presence impossible.

A variety of forms of rescue helmets and apparatus are on the market, almost all of European manufacture, which are being subjected to comparative trials as to their durability and safety, the ease or inconvenience involved in their use, etc. All consist essentially of helmets which fit air-tight about the head, or of air-tight nose clamps and mouthpieces (Fig. 1, Plate XII).

These several forms of breathing apparatus are of three types:

1.—The liquid-air type, in which air, in a liquid state, evaporates and provides a constant supply of fresh air.

2.—The chemical oxygen-producing type, which artificially makes or supplies oxygen for breathing at about the rate required; and,

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FIG. 1.—BREATHING AND RESCUE APPARATUS.

FIG. 2.—RESCUE TRAINING ROOM.

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3.—The compressed-oxygen type.

Apparatus of the first type, weighing 20 lb., supplies enough air to last about 3 hours, and the products of breathing pass through a check-valve directly into space. Apparatus of the second type supplies oxygen obtained from oxygen-producing chemicals, and also provides means of absorbing the carbonic acid gas produced in respiration. They contain also the requisite tubes, valves, connections, etc., for the transmission of the fresh air and the respired air so as to produce sufficient oxygen while in use; to absorb and purify the products of expiration; and to convey the fresh air to the mouth without contamination by the atmosphere in which the apparatus is used. Three oxygen-generating cartridges are provided, each supplying oxygen enough for 1 hour, making the total capacity 3 hours. Changes of cylinders can be made in a few seconds while breathing is suspended. This apparatus weighs from 20 to 25 lb., according to the number of oxygen generators carried. The cartridges for generating oxygen, provided with this apparatus, are of no value after having been used for about an hour.

The third type of apparatus is equipped with strong cylinders charged with oxygen under high pressure; two potash regenerative cans for absorbing the carbon dioxide gas exhaled; a facial helmet; the necessary valves, tubes, etc., for the control of the oxygen; and a finimeter which registers the contents of the cylinders in atmospheres and minutes of duration. The two cartridges used for absorbing the carbonic acid gas are of no value after having been in use for two hours.

If inhalation is through the mouth alone, a mouthpiece is attached to the end of the breathing tube by which the air or oxygen is supplied, the nose is closed by a clip, and the eyes are protected by goggles. To inhale through both nose and mouth, the miner wears a helmet or headgear which can be made to fit tightly around the face. The helmet has two tubes attached, one for inspiration and the other for expiration. In the oxygen-cylinder apparatus these tubes lead to and from rubber sacks used for pure-air and bad-air reserves.

Mine-Rescue Training.

It has been found in actual service that when a miner, equipped with breathing apparatus for the first time, enters a mine in which an explosion has occurred, he is soon overcome by excitement or

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nervousness induced by the artificial conditions of breathing imposed by the apparatus, the darkness and heat, and the consciousness that he is surrounded with poisonous gases. It has also been found that a brief period of training in the use of such apparatus, under conditions simulating those encountered in a mine after a disaster, gives the miner confidence and enables him to use the apparatus successfully under the strain of the vigorous exertion incident to rescue work.

The rescue corps consists of five or six miners under the direction of a mining engineer who is experienced in rescue operations and familar with the conditions existing after mine disasters. The miners work in pairs, so that one may assist the other in case of accident, or of injury to the breathing apparatus, and so that each may watch the condition of the oxygen supply, as shown by the gauges in the other's outfit.

The training is given in the gas-tight room of Building No. 17, or in similar rooms at sub-stations (Fig. 2, Plate XII). This room is made absolutely dark, and is filled with formaldehyde gas, SO_2, CO_2, or CO, produced by burning sulphur or charcoal on braziers. At each period of training, the miners enter and walk a distance of about 1 mile, the average distance usually traveled from the mine mouth to the working face or point of explosion. They then remove a number of timbers; lift a quantity of brick or hard lump-coal into wheel-barrows; climb through artificial tunnels, up and down inclines, and over surfaces strewn with coal or stone; operate a machine with a device attached to it, which automatically records the foot-pounds of work done; and perform other vigorous exercise, during a period of 2 hours. This routine is repeated daily during 1 week, after which the rescue corps is considered sufficiently trained for active service.

The apparatus used for recording the foot-pounds of work done by the person operating the work machine within the gas-tight rescue room, comprises a small dial with electrical connections, which records the number of strokes made by the machine, and a pencil point which rests on a paper diaphragm, fastened to a horizontal brass disk. This disk is driven by clockwork, and makes one complete revolution per hour. When the machine is in operation, the pencil point works back and forth, making a broad line on the paper; when the operator of the machine rests, the pencil point traces a single line. The

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apparatus thus records the number of strokes given by the operator during a given time. From the weight lifted, the height of lift, and the number of strokes in the given time, the foot-pounds of work are readily calculated.

Electric Testing Apparatus.

On the ground floor of Building No. 10, two rooms are occupied as laboratories for investigating the electrical equipment used in mining operations. The purpose of these investigations is to ascertain the conditions under which electricity of various voltages may be used with safety—in mine haulage, hoisting, pumping, or lighting—in the presence of dangerous mixtures of explosive gases or of dust. It is also proposed to test various kinds of insulation and insulators in this laboratory, and to determine the durability of such insulation in the presence of such corrosive gases and water as are found in mines.

A water-proof wooden tank, measuring 15 by 5 by 5 ft., is installed, in which insulation and insulating materials are tested under either pure or polluted water. Various electric lighting devices and equipment can be connected from a switch-board in Building No. 17 with Gas-and-Dust Gallery No. 2, for testing the effect of such lighting apparatus in the presence of explosive mixtures of gas and dust, as set forth on page 220.

In the electrical laboratory, Building No. 10, is a booster set developing 60 kw., and an appropriate switch-board for taking direct current at 220 volts from the turbo-generator and converting it into current varying from 0 to 750 volts. There are also transformers for developing 60-cycle, alternating current at voltages of from 110 to 2 200. The switch-board is designed to handle these various voltages and to communicate them to the apparatus under test in Building No. 10, Gallery No. 2, or elsewhere.

Tests are in progress of insulating materials for use in mines, and of electric fuses, lights, etc., in Gallery No. 2 (Fig. 3, Plate X), and in the lamp-testing box (Fig. 2, Plate XI). It is proposed, at the earliest possible date, to make comparative tests of the safety of various mine locomotives and mine-hoisting equipment through the medium of this laboratory, and it is believed that the results will furnish valuable information as a guide to the safety, reliability, and durability of these appliances when electrically operated.

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Electric Lamp and Fuse Testing Box.—An apparatus for testing safety lamps and electric lights and fuses, consists of ¼-in. iron plates, bolted together with 1½-in. angle-irons to form a box with inside dimensions of 18 by 18 by 24 in. The box is placed on a stand at such a height that the observation windows are on a level with the observer's eye (Fig. 2, Plate XI), and it is connected, by a gas-pipe, with a supply of natural gas which can be measured by a gas-holder or meter alongside the box.

By the use of this apparatus the effect of explosive gas on flames, of electric sparks on explosive mixtures of gas and air, and of breaking electric lamps in an explosive mixture of gas and air, may be studied. The safety lamps are introduced into the box from beneath, through a hole 6 in. square, covered with a hinged iron lid, admission to which is had through a flexible rubber sleeve, 20 in. long.

The behavior of the standard safety lamp and of the safety lamps undergoing test may be compared in this box as to height of flame for different percentages of methane in the air, the effect of such flames in igniting gas, etc.

In each end of the box is an opening 1 ft. square, over which may be placed a paper diaphragm held by skeleton doors, the purpose of which is to confine the gas in such a manner that, should an explosion occur, no damage would be done. In the front of the box are two plate-glass observing windows, 2⅝ by 5½ in. In the side of the box, between the two windows, is a ⅜-in. hole, which can be closed by a tap-screw, through which samples for chemical analysis are drawn.

The gasometer consists of two iron cans, the lower one being open at the top and filled with water and the upper one open at the bottom and suspended by a counterweight. The latter has attached to its upper surface a scale which moves with it, thereby measuring the amount of gas in the holder. A two-way cock permits the admission of gas into the gasometer and thence into the testing box.

Gas-and-Dust Gallery No. 2.—This gallery is constructed of sheet steel and is similar to Gallery No. 1, the length, however, being only 30 ft. and the diameter 10 ft. It rests on a concrete foundation (Fig. 3, Plate X). Diaphragms can be placed across either extremity, or at various sections, to confine the mixtures of gas and air in which the tests are made. The admission of gas is controlled by pipes and valves, and the gas and air can be stirred or mixed by a fan, as described for Gallery No. 1, and as shown by Fig. 1.

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Gallery No. 2 is used for investigating the effect of flames of various lamps, of electric currents, motors, and coal-cutting machines, in the presence of known mixtures of explosive gas and air. It is also used for testing the length of flame of safety lamps in still air carrying various proportions of methane, and, for this purpose, is more convenient than the lamp gallery. In tests with explosive mixtures, after the device to be tested has been introduced and preparations are completed, operations are controlled from a safe distance by a switch-board in a building near-by.

Among other investigations conducted in this gallery are those of the effect of sparks on known gas mixtures. These sparks are such as those struck from a pick on flint, but in this case they are produced by rubbing a rapidly revolving emery wheel against a steel file. The effect of a spark produced by a short circuit of known voltage, the flame from an arc lamp, etc., may also be studied in this gallery.

Structural Materials Investigations.

The structural materials investigations are being conducted for the purpose of determining the nature and extent of the materials available for use in the building and construction work of the Government, and how these materials may be used most efficiently.

These investigations include:

(1).—Inquiries into the distribution and local availability, near each of the building centers in the United States, of such materials as are needed by the Government.

(2).—How these materials may be used most efficiently.

(3).—Their fire-resisting qualities and strength at different temperatures.

(4).—The best and most economic methods of protecting steel by fire-resistant covering.

(5).—The most efficient methods of proportioning and mixing the aggregate, locally available, for different purposes.

(6).—The character and value of protective coatings, or of various mixes, to prevent deterioration by sea water, alkali, and other destructive agencies.

(7).—The kinds and forms of reinforcement for concrete necessary to secure the greatest strength in beams, columns, floor slabs, etc.

(8).—Investigation of the clays and of the products of clays needed in Government works, as to their strength, durability, suitability as

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fire-resisting materials, and the methods of analyzing and testing clay products.

(9).—Tests of building stones, and investigations as to their availability near the various building centers throughout the United States.

The operations of the Structural Materials Division include investigations into cement-making materials, constituent materials of concrete, building stones, clays, clay products, iron, steel, and miscellaneous materials of construction, for the use of the Government. The organization comprises a number of sections, including those for the chemical and physical examination of Departmental purchases; field sampling and laboratory examination of constituent materials of concrete collected by skilled field inspectors in the neighborhood of the larger commercial and building centers; similar field sampling of building stones and of clays and clay products, offered for use in Government buildings or engineering construction; and the forwarding of such samples to the testing laboratories at St. Louis or Pittsburg for investigation and test. The investigative tests include experiments regarding destructive agencies, such as electrolysis, alkaline earths and waters, salt water, fire, and weathering; also experiments with protective and water-proofing agencies, including the various washes or patented mixtures on the market, and the methods of washing, and mixing mortars and concrete, which are likely to result in rendering such materials less pervious to water.

Investigations are also being conducted to determine the nature and extent of materials available for use in the building-construction work of the Government, and how these materials may be used most efficiently and safely. While the act authorizing this work does not permit investigations or tests for private parties, it is believed that these tests for the Government cannot fail to be of great general value. The aggregate expenditure by the Federal Government in building and engineering construction is about $40 000 000 annually. This work is being executed under so many different conditions, at points so widely separated geographically, and requires so great a variety of materials, that the problems to be solved for the Government can hardly fail to cover a large share of the needs of the Engineering Profession, State and municipal governments, and the general public.

Character of the Work.—The tests and analyses, of the materials of construction purchased by the various bureaus and departments for the use of the Government, are to determine the character, quality,

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suitability, and availability of the materials submitted, and to ascertain data leading to more accurate working values as a basis for better working specifications, so as to enable Government officials to use such materials with more economy and increased efficiency.

Investigative tests of materials entering into Government construction, relative to the larger problems involved in the use of materials purchased by the Government, include exhaustive study of the suitability for use, in concrete construction on the Isthmian Canal, of the sand and stone, and of the cementing value of pozzuolanic material, found on the Isthmus; the strength, elasticity, and chemical properties of structural steel for canal lock-gates; of wire rope and cables for use in hoisting and haulage; and the most suitable sand and stone available for concrete and reinforced concrete for under-water construction, such as the retaining walls being built by the Quartermaster's Department of the Army, in San Francisco Harbor.

These tests also include investigations into the disintegrating effect of alkaline soil and water on the concrete and reinforced concrete structures of the Reclamation Service, with a view to preventing such disintegration; investigations into the proper proportions and dimensions of concrete and reinforced concrete structural columns, beams, and piers, and of walls of brick and of building stone, and of the various types of metal used for reinforcement by the Supervising Architect in the construction of public buildings; investigations into the sand, gravel, and broken stone available for local use in concrete construction, such as columns, piers, arches, floor slabs, etc., as a guide to the more economical design of public structures, and to determine the proper method of mixing the materials to render the concrete most impervious to water and resistant to weather and other destructive agencies.

Other lines of research may be stated briefly as follows:

The extent to which concrete made from cement and local materials can be most safely and efficiently used for different purposes under different conditions;

The best methods for mixing and utilizing the various constituent materials locally available for use in Government construction;

The materials suitable for the manufacture of cement on the public lands, or where the Government has planned extensive building or engineering construction work, where no cement plants now exist;

The kinds and forms of reinforcement for concrete, and the best

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methods of applying them in order to secure the greatest strength in compression, tension, shear, etc., in reinforced concrete beams, columns, floor slabs, etc.;

The influence of acids, oils, salts, and other foreign materials, long-continued strain, or electric currents, on the permanence of the steel in reinforced concrete;

The value of protective coatings as preventives of deterioration of structural materials by destructive agencies; and

The establishment of working stresses for various structural materials needed by the Government in its buildings.

Investigations are being made into the effects of fire and the rate of conductivity of heat on concrete and reinforced concrete, brick, tile, building stone, etc., as a guide to the use of the most suitable materials for fire-proof building construction and the proper dimensioning of fire-resistive coverings.

Investigations and tests are being made, with a view to the preparation of working specifications for use in Government construction, of bricks, tile, sand-lime brick, paving brick, sewer pipe, roofing slates, flooring tiles, cable conduits, electric insulators, architectural terra cotta, fire-brick, and all shapes of refractories and other clay products, regarding which no satisfactory data for the preparation of specifications of working values now exist.

Investigations of the clay deposits throughout the United States are in progress, to determine proper methods of converting them into building brick, tile, etc., at the most reasonable cost, and the suitability of the resulting material for erection in structural forms and to meet building requirements.

Investigations are being made in the field, of building stones locally available, and physical and chemical tests of these building stones to determine their bearing or crushing strength; the most suitable mortars for use with them; their resistance to weathering; their fire-resistive and fire-proof qualities, etc., regarding which practically no adequate information is available as a guide to Government engineering and building design.

Results Accomplished.—During one period of six months alone, more than 2 500 samples, taken from Government purchases of structural materials, were examined, of which more than 300 failed to meet the specified requirements, representing many thousands of dollars worth of inferior material rejected, which otherwise would have been

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paid for by the Government. These tests were the means of detecting the inferior quality of large quantities of materials delivered on contracts, and the moral effect on bidders has proven as important a factor in the maintenance of a high quality of purchases, as in the saving of money.

The examination of sands, gravels, and crushed stones, as constituent materials for concrete and reinforced concrete construction, has developed data showing that certain materials, locally available near large building centers and previously regarded as inferior in quality, were, in fact, superior to other and more expensive materials which it had been proposed to use.

These investigations have represented an actual saving in the cost of construction on the work of the Isthmian Canal Commission, of the Supervising Architect, and of certain States and cities which have benefited by the information disseminated regarding these constituent materials.

Investigations of clay products, only recently inaugurated, have already resulted in the ascertainment of important facts relative to the colloid matter of clay and its measurement, and the bearing thereof on the plasticity and working values of various clays. The study of the preliminary treatment of clays difficult to handle dry, has furnished useful information regarding the drying of such clays, and concerning the fire resistance of bricks made of soft, stiff, or dried clay of various densities.

The field collection and investigation of building-stone samples have developed some important facts which had not been considered previously, relative to the effect of quarrying, in relation to the strike and dip of the bedding planes of building stone, and the strength and durability of the same material when erected in building construction. These investigations have also developed certain fundamental facts relative to the effects of blasting (as compared with channeling or cutting) on the strength and durability of quarried building stone.

Mineral Chemistry Laboratories.—Investigations and analyses of the materials of engineering and building construction are carried on at Pittsburg in four of the larger rooms of Building No. 21. In this laboratory, are conducted research investigations into the effect of alkaline waters and soils on the constituent materials of concrete available in arid regions, as related to the life and permanency of the concrete and reinforced concrete construction of the Reclamation Service.

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These investigations include a study of individual salts found in particular alkalis, and a study of the results of allowing solutions of various alkalis to percolate through cylinders of cement mortar and concrete. Other research analyses have to do with the investigation of destructive and preservative agencies for concrete, reinforced concrete, and similar materials, and with the chemistry of the effects of salt water on concrete, etc. The routine chemical analyses of the constituent materials of concrete and cement-making materials, are made in this laboratory, as are also a large number of miscellaneous chemical analyses and investigations of reinforcement metal, the composition of building stones, and allied work.

A heat laboratory, in charge of Dr. J. K. Clement, occupies three rooms on the ground floor of Building No. 21, and is concerned chiefly with the measurement of temperatures in gas producers, in the furnaces of steam boilers, kilns, etc. The work includes determinations of the thermal conductivity of fire clays, concrete, and other building materials, and of their fire-resisting properties; measurements of the thermal expansion and specific heats of fire-bricks, porcelain, and glazes; and investigations of the effect of temperature variations on the various chemical processes which take place in the fuel bed of the gas producer, boiler furnace, etc.

The heat laboratory is equipped for the calibration of the thermometers and pyrometers, and electrical and other physical apparatus used by the various sections of the Technologic Branch.

For convenience in analyzing materials received from the various purchasing officers attached to the Government bureaus, this work is housed in a laboratory on the fourth floor of the Geological Survey Building in Washington.

Large quantities and many varieties of building materials for use in public buildings under contract with the Supervising Architect's office, are submitted to the laboratory by contractors to determine whether or not they meet the specified requirements. Further examinations are made of samples submitted by superintendents of construction, representing material actually furnished by contractors. It is frequently found that the sample of material submitted by the contractor is of far better quality than that sent by the superintendent to represent deliveries. The needed constant check on deliveries is thus provided.

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In addition to this work for the office of the Supervising Architect, similar work on purchases and supplies is carried on for the Isthmian Canal Commission, the Quartermaster-General's Department of the Army, the Life Saving Service, the Reclamation Service, and other branches of the Government. About 300 samples are examined each month, requiring an average of 12 determinations per sample, or about 3 600 determinations per month.

The chemical laboratory for testing Government purchases of structural materials is equipped with the necessary apparatus for making the requisite physical and chemical tests. For the physical tests of cement, there are a tensile test machine, briquette moulds, a pat tank for boiling tests to determine soundness, water tanks for the storage of briquettes, a moist oven, apparatus to determine specific gravity, fineness of grinding, etc.

The chemical laboratory at Washington is equipped with the necessary analytical balances, steam ovens, baths, blast lamps, stills, etc., required in the routine chemical analysis of cement, plaster, clay, bricks and terra cotta, mineral paints and pigments, roofing material, tern plate and asphaltic compounds, water-proofing materials, iron and steel alloys, etc.

At present, materials which require investigative tests as a basis for the preparation of suitable specifications, tests not connected with the immediate determination as to whether or not the purchases are in accordance with the specifications, are referred to the chemical laboratories attached to the Structural Materials Division, at Pittsburg.

The inspection and tests of cement purchased in large quantities, such as the larger purchases on behalf of public-building construction under the Supervising Architect, or the great 4 500 000-bbl. contract of the Isthmian Canal Commission, are made in the cement-testing laboratory of the Survey, in the Lehigh Portland cement district, at Northampton, Pa.

Testing Machines.—The various structural forms into which concrete and reinforced concrete may be assembled for use in public-building construction, are undergoing investigative tests as to their compressive and tensile strength, resistance to shearing, modulus of elasticity, coefficient of expansion, fire-resistive qualities, etc. Similar tests are being conducted on building stone, clay products, and the structural forms in which steel and iron are used for building construction.

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The compressive, tensile, and other large testing machines, for all kinds of structural materials reaching the testing stations, are under the general supervision of Richard L. Humphrey, M. Am. Soc. C. E. The immediate direction of the physical tests on the larger testing machines is in charge of Mr. H. H. Kaplan.

Most of this testing apparatus, prior to 1909, was housed in buildings loaned by the City of St. Louis, in Forest Park, St. Louis, Mo., and the arrangement of these buildings, details of equipment, organization, and methods of conducting the tests, are fully set forth in Bulletin No. 329 of the U. S. Geological Survey. In brief, this equipment included motor-driven, universal, four-screw testing machines, as follows: One 600 000-lb., vertical automatic, four-screw machine; one 200 000-lb., automatic, four-screw machine; and one 200 000-lb. and one 100 000-lb. machine of the same type, but with three screws. There are a number of smaller machines of 50 000, 40 000, 10 000, and 2 000 lb., respectively.

These machines are equipped so that all are available for making tensile and compressive tests (Fig. 1, Plate XIII). The 600 000-lb. machine is capable of testing columns up to 30-ft. lengths, and of making transverse tests of beams up to 25-ft. span, and tension tests for specimens up to 24 ft. in length. The smaller machines are capable of making tension and compressive tests up to 4 ft. in length and transverse beam tests up to 12 ft. span. In addition, there are ample subsidiary apparatus, including concrete mixers with capacities of ½ and 1 cu. yd., five hollow concrete block machines, automatic sifting machines, briquette moulds, storage tanks, etc.

At the Atlantic City sub-station, there is also a 200 000-lb., universal, four-screw testing machine, with miscellaneous equipment for testing cement and moulding concrete, etc.; and at the Northampton sub-station, there is a complete equipment of apparatus for cement testing, capable of handling 10 000 bbl. per day.

At the Pittsburg testing station, a 10 000 000-lb., vertical, compression testing machine (Plate XIV), made by Tinius Olsen and Company, is being erected for making a complete series of comparative tests of various building stones of 2, 4, and 12-in. cube, of stone prisms, 12 in. base and 24 in. high, of concrete and reinforced concrete columns up to 65 ft. in height, and of brick piers and structural-steel columns up to the the limits of the capacity and height of the machine.

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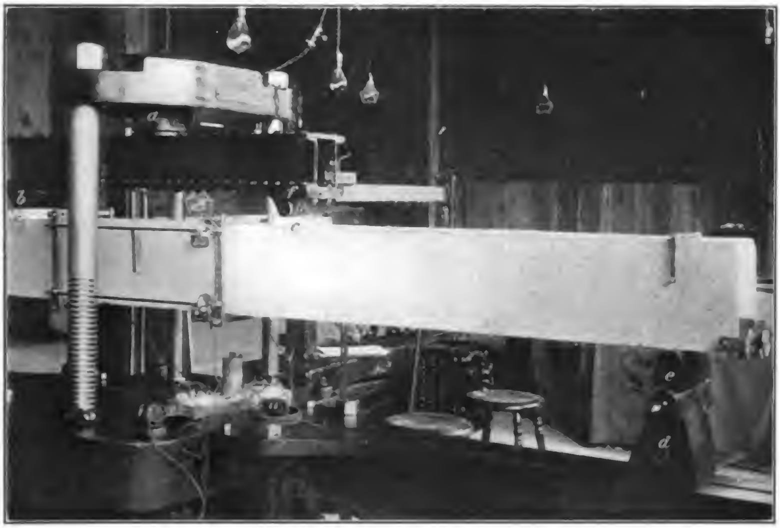

FIG. 1.—TESTING BEAM IN 200 000-LB. MACHINE.

FIG. 2.—FIRE TEST OF PANEL.

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This machine is a large hydraulic press, with an adjustable head, and a weighing system for recording the loading developed by a triple-plunger pump. It has a maximum clearance of 65 ft. between heads; the clearance in the machine is a trifle more than 6 ft. between screws, and the heads are 6 ft. square.

The machine consists of a base containing the main cylinder, with a sectional area of 2 000 sq. in., upon which rests the lower platform or head, which is provided with a ball-and-socket bearing. The upper head is adjustable over four vertical screws, 13½ in. in diameter and 72 ft. 2 in. long, by a system of gearing operating four nuts with ball-bearings upon which the head rests. The shafting operating this mechanism is connected with a variable-speed motor which actuates the triple-plunger pump supplying the pressure to the main cylinder (Fig. 4).

The weighing device consists of a set of standard Olsen levers for weighing one-eightieth of the total load on the main cylinder. This reduction is effected through the medium of a piston and a diaphragm. The main cylinder has a diameter of 50 in., and the smaller one, a diameter of $5\frac{9}{16}$ in. The weighing beam is balanced by an automatically-operated poise weight, and is provided with a device for applying successive counterweights of 1 000 000 lb. each. Each division on the dial is equivalent to a 100-lb. load, and smaller subdivisions are made possible by an additional needle-beam.

The power is applied by a 15-h.p., 220-volt, variable-speed motor operating a triple-plunger pump, the gearing operating the upper head being driven by the same motor. The extreme length of the main screws necessitates splicing, which is accomplished as follows:

In the center of the screws, at the splice, is a 3-in. threaded pin for centering the upper and lower screws; this splice is strengthened by sleeve nuts, split to facilitate their removal whenever it is necessary to lower the upper head; after the head has passed the splice, the sleeve nuts are replaced.

In order to maintain a constant load, a needle-valve has been provided, which, when the pump is operated at its lowest speed, will allow a sufficient quantity of oil to flow into the main cylinder to equalize whatever leakage there may be. The main cylinder has a vertical movement of 24 in. The speed of the machine, for the purpose of adjustment, using the gearing attached to the upper head, is 10 in. per

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min. The speed for applying loads, controlled by the variable-speed motor driving the pump, varies from a minimum of at least $\frac{1}{60}$ in. per min. to a maximum of at least ½ in. per min. The machine has a guaranteed accuracy of at least one-third of 1%, for any load of more than 100 000 lb., up to its capacity.

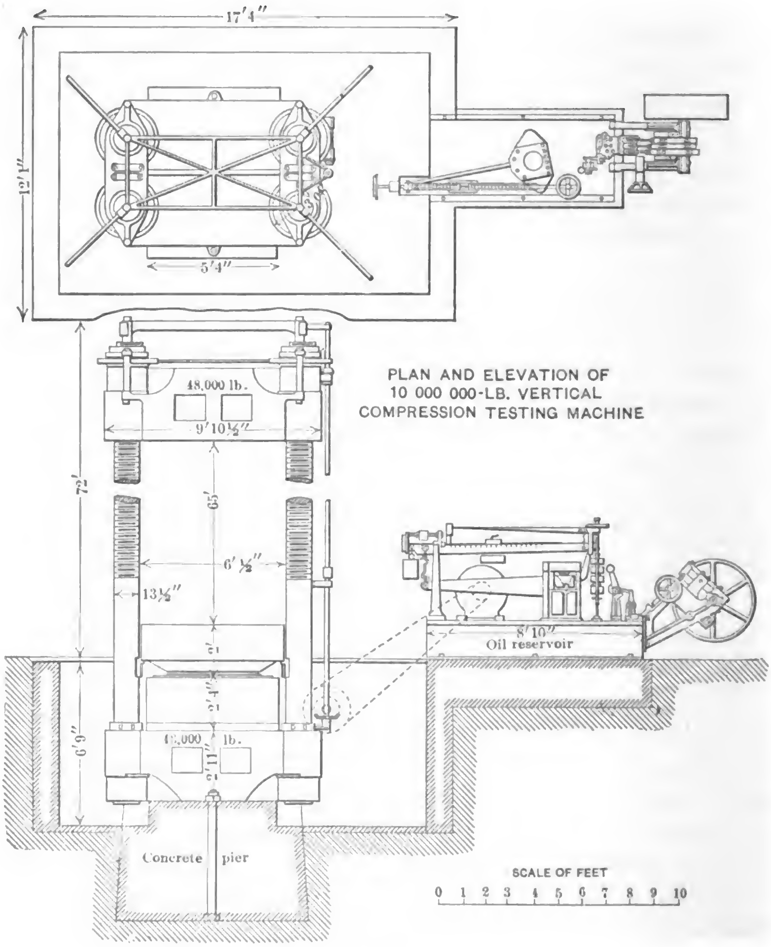

FIG. 4.

The castings for the base and the top head weigh approximately 48 000 lb. each. Each main screw weighs more than 40 000 lb., the lower platform weighing about 25 000 lb., and the main cylinder, 16 000 lb. The top of the machine will be about 70 ft. above the top

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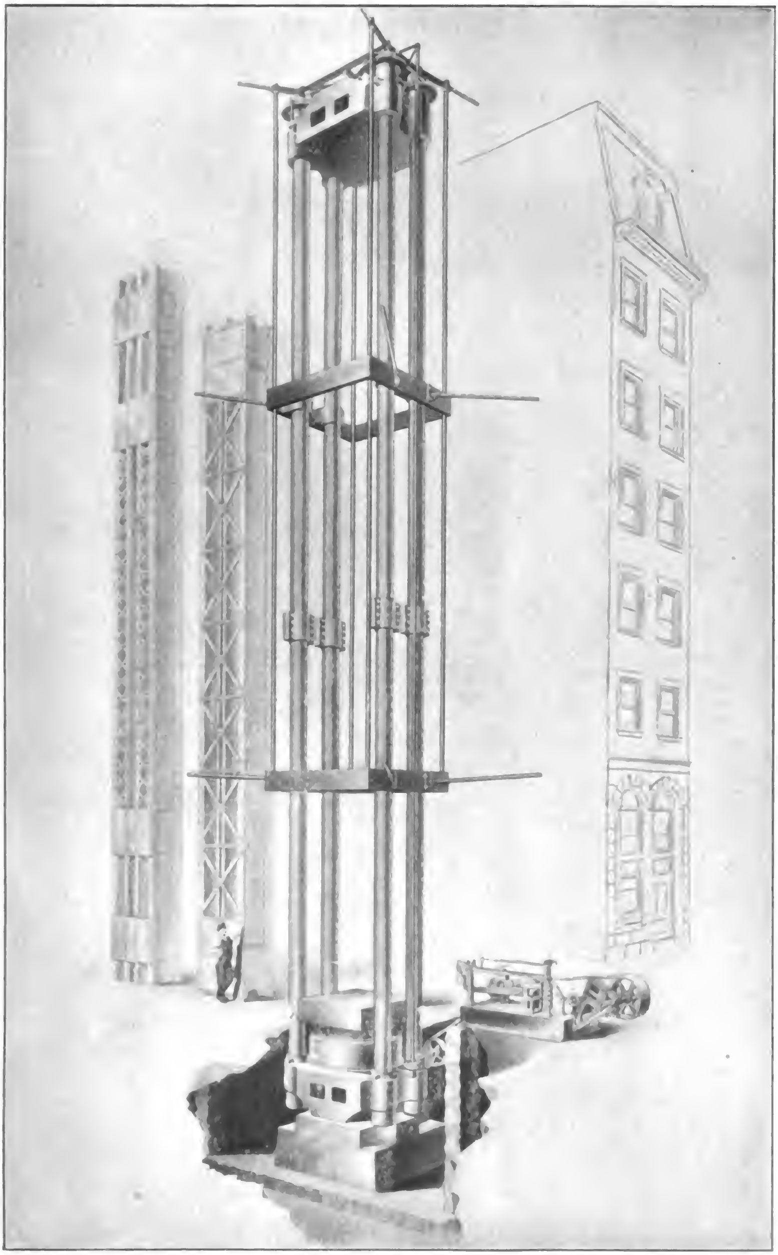

10 000 000-Lb. Testing Machine.

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of the floor, and the concrete foundation, upon which it rests, is about 8 ft. below the floor line.

Concrete and Cement Investigations.—The investigations relating to concrete include the examination of the deposits of sand, gravel, stone, etc., in the field, the collection of representative samples, and the shipment of these samples to the laboratory for analysis and test. These tests are conducted in connection with the investigation of cement mortars, made from a typical Portland cement prepared by thoroughly mixing a number of brands, each of which must meet the following requirements:

Specific gravity, not less than 3.10;

Fineness, residue not to exceed 8% on No. 100, nor 25% on No. 200 sieve;

Time of setting: Initial set, not less than 30 min.; hard set, not less than 1 hour, nor more than 10 hours.

Tensile strength: Requirements applying to neat cement and to 1 part cement with 3 parts standard sand:

Time specification.	Neat cement. Pounds.	1:3 Mix. Pounds.
24 hours in moist air	175	...
7 days (1 day in moist air, 6 days in water)	500	175
28 days (1 day in moist air, 27 days in water)	600	250

Constancy of volume: Pats of neat cement, 3 in. in diameter, ½ in. thick at center, tapering to a thin edge, shall be kept in moist air for a period of 24 hours. A pat is kept in air at normal temperature and observed at intervals for at least 28 days. Another pat is kept in water maintained as near 70° Fahr. as practicable, and is observed at intervals for at least 28 days. A third pat is exposed in an atmosphere of steam above boiling water, in a loosely-closed vessel, for 5 hours. These pats must remain firm and hard and show no signs of distortion, checking, cracking, or disfiguration.

The cement shall not contain more than 1.75% of anhydrous sulphuric acid, nor more than 4% of magnesium oxide.

A test of the neat cement must be made with each mortar series for comparison of the quality of the typical Portland cement.

The constituent materials are subjected to the following examination and determinations, and, in addition, are analyzed to determine the composition and character of the stone, sand, etc.:

1.—Mineralogical examination,

2.—Specific gravity,

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3.—Weight, per cubic foot,

4.—Sifting (granulometric composition),

5.—Percentage of silt and character of same,

6.—Percentage of voids,

7.—Character of stone as to percentage of absorption, porosity, permeability, compressive strength, and behavior under treatment.

Physical tests are made to determine the tensile, compressive, and transverse strengths of the cement and mortar test pieces, with various preparations of cement and various percentages of material. Tests are also made to determine porosity, permeability, volumetric changes in setting, absorption, coefficient of expansion, effect of oil, etc.

Investigation of concretes made from mixtures of typical Portland cement, sand, stone, and gravel, includes tests on cylinders, prisms, cubes, and other standard test pieces, with various proportions of materials and at ages ranging from 30 to 360 days. Full-sized plain concrete beams, moulded building blocks, reinforced concrete beams, columns, floor slabs, arches, etc., are tested to determine the effect, character, and amount of reinforcement, the effect of changes in volume, size, and composition, and the effect of different methods of loading and of supporting these pieces, etc.

These investigations include detailed inquiry in the field and research in the chemical and physical laboratories regarding the effects of alkaline soils and waters on structures of concrete being built by the Reclamation Service in the arid regions. It has been noted that on certain of the Reclamation projects, notably on the Sun River Project, near Great Falls, Mont., the Shoshone Project, near Cody, Wyo., and the Carlsbad and Hondo Projects in the Pecos Valley, N. Mex., structures of concrete, reinforced concrete, building stones, brick, and tile, show evidence of disintegration. This is attributed to the effects of alkaline waters or soils coming into contact with the structures, or to the constituent materials used. In co-operation with the Reclamation Service, samples of the waters, soils, and constituent materials, are collected in the field, and are subjected to careful chemical examination in the mineral laboratories at Pittsburg.

The cylinders used in the percolation tests are composed of typical Portland cement mixed with sand, gravel, and broken stone of known composition and behavior, and of cement mixed with sand, gravel, and

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FIG. 1.—CHARACTERISTIC FAILURES OF REINFORCED CONCRETE BEAMS.

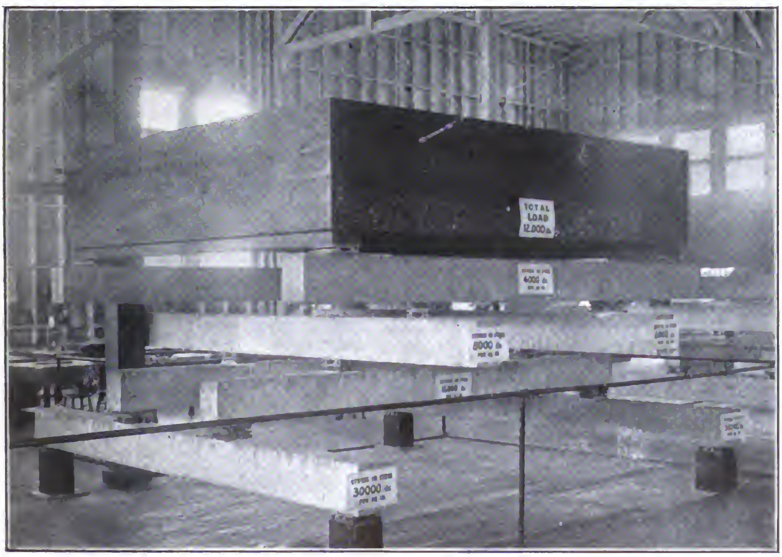

FIG. 2.—ARRANGEMENT OF STATIC LOAD TEST FOR REINFORCED CONCRETE BEAMS.

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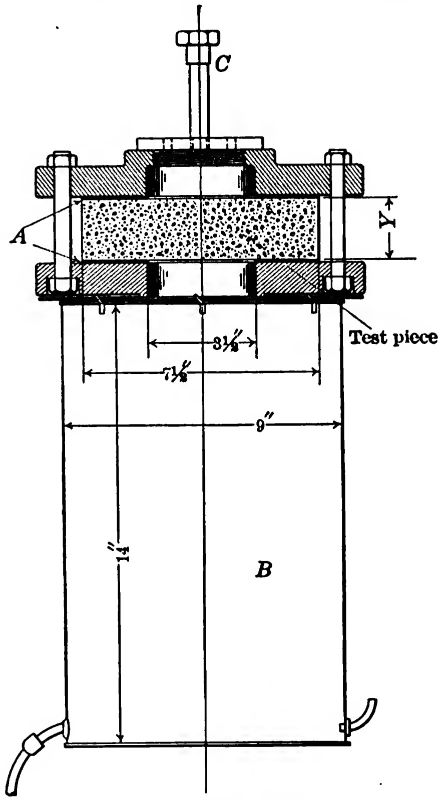
broken stone collected in the neighborhood of the Reclamation projects under investigation.

It is also proposed to subject these test pieces, some made with water of known purity, and others with alkaline water, to contact with alkaline soils near the projects, and with soil of known composition near the testing laboratories at Pittsburg. As these tests progress and other lines of investigation are developed, the programme will be extended, in the hope that the inquiry may develop methods of preparing and mixing concrete and reinforced concrete which can be used in alkaline soils without danger of disintegration.

Investigations into the effect of salt water on cement mortars and concretes, and the effect of electrolysis, are being conducted at Atlantic City, N. J., where the test pieces may be immersed in deep sea water for longer or shorter periods of time.



CROSS-SECTION OF APPARATUS FOR HOLDING PERMEABILITY-TEST PIECES.

FIG. 5.

At the Pittsburg laboratory a great amount of investigative work is done for the purpose of determining the suitability and availability of various structural materials submitted for use by the Government. While primarily valuable only to the Government, the results of these tests are of indirect value to all who are interested in the use of similar materials. Among such investigations have been those relating to the strength, elasticity, and chemical properties of wire rope for use in the Canal Zone; investigations of the suitability and cementing value of con-

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crete, sand, stone, and pozzuolanic material found on the Isthmus; investigations as to the relative resistance to corrosion of various types of wire screens for use in the Canal Zone; into the suitability for use, in concrete sea-wall construction, of sand and stone from the vicinity of San Francisco; into the properties of reinforced concrete floor slabs; routine tests of reinforcing metal, and of reinforced concrete beams and columns, for the Supervising Architect of the Treasury Department, etc. The results have been set forth in three bulletins* which describe the methods of conducting these tests and also tests on constituent materials of concrete and plain concrete beams. In addition, there are in process of publication a number of bulletins giving the results of tests on reinforced concrete beams, columns, and floor slabs, concrete building blocks, etc.

The Northampton laboratory was established because it is in the center of the Lehigh cement district, and therefore available for the mill sampling and testing of purchases of cement made by the Isthmian Canal Commission; it is also available for tests of cement purchased in the Lehigh district by the Supervising Architect and others. It is in a building, the outer walls of which are of cement plaster applied over metal lath nailed to studding. The partitions are of the same construction, and the floors and roof are of concrete throughout.

The inspection at the factories and the sampling of the cement are under the immediate direction of the Commission; the testing is under the direction of the U. S. Geological Survey. A large force of employees is required, in view of the magnitude of the work, which includes the daily testing of consignments ranging from 5 000 to 10 000 bbl., sampled in lots of 100 bbl., which is equivalent to from 50 to 100 samples tested per day.

The cement to be sampled is taken from the storage bins and kept under seal by the chief inspector pending the results of the test. The quantity of cement sampled is sufficient for the tests required under the specifications of the Isthmian Canal Commission, as well as for preliminary tests made by the cement company, and check tests made at the Geological Survey laboratory, at Pittsburg.

The tests specified by the Commission include determination of

* "Structural Materials Testing Laboratories," by Richard L. Humphrey. Bulletin No. 329. U. S. Geological Survey, 1908; "Portland Cement Mortars and their Constituent Materials," by Richard L. Humphrey and William Jordan, Jr., Bulletin No. 331, U. S. Geological Survey, 1908; "Strength of Concrete Beams," by Richard L. Humphrey, Bulletin No. 344, U. S. Geological Survey, 1908.

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specific gravity, fineness of grinding, time of setting, soundness, tensile strength (with three parts of standard quartz sand for 7 and 28 days, respectively), and determination of sulphur anhydride (SO_3), and magnesia (MgO).

The briquette-making and testing room is fitted with a mixing table, moist closet, briquette-storage tanks, and testing machines. The mixing table has a concrete top, in which is set plate glass, 18 in. square and 1 in. thick. Underneath the table are shelves for moulds, glass plates, etc.

The moist closet, 5 ft. high, 3 ft. 10 in. wide, and 1 ft. 8 in. deep, is divided into two compartments by a vertical partition, and each compartment is fitted with cleats for supporting thirteen tiers of glass plates. On each pair of cleats, in each compartment, can be placed four glass plates, each plate containing a 4-gang mould, making storage for 416 briquettes. With the exception of the doors, which are of wood lined with copper, the closet is of 1:1 cement mortar, poured monolithic, even to the cleats for supporting the glass plates.

The immersion tanks, of the same mortar, are in tiers of three, supported by a steel structure. They are 6½ ft. long, 2½ ft. wide, and 6 in. deep, and 2 000 briquettes can be stored in each tank. The overflow from the top tank wastes into the second, which, in turn, wastes into the third. Water is kept running constantly.

The briquette-testing machine is a Fairbanks shot machine with a capacity of 2 000 lb., and is regulated to apply the load at the rate of 600 lb. per min. Twenty-four 4-gang moulds, of the type recommended by the Special Committee on Uniform Tests of Cement, of the American Society of Civil Engineers, are used.

The room for noting time of set and soundness is fitted with a mixing table similar to that in the briquette-making room. The Vicat apparatus is used for determining the normal consistency, and the Gilmore apparatus for the time of setting. While setting, the soundness pats are stored in galvanized-iron pans having about 1 in. of water in the bottom, and covered with dampened felt or burlap. The pats rest on a rack slightly above the water and well below the felt.

For specific gravity tests, the Le Chatelier bottles are used. A pan, in which five bottles can be immersed at one time, is used for maintaining the benzine at a constant temperature. The samples are weighed on a pair of Troemner's No. 7 scales.

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The fineness room is fitted with tables, two sets of standard No. 100 and No. 200 sieves, and two Troemner's No. 7 scales similar to those used for the specific gravity tests.

The storage room is fitted with shelves for the storage of samples being held for 28-day tests.

The mould-cleaning room contains tables for cleaning moulds, and racks for air pats.

An effort is made to keep all the rooms at a temperature of 70° Fahr., and, with this in view, a Bristol recording thermometer is placed in the briquette-room. Two wet-and-dry bulb hygrometers are used to determine the moisture in the air.

Samples are taken from the conveyor which carries the cement to the storage bins, at the approximate rate of one sample for each 100 bbl. After each 4000-bbl. bin has been filled, it is sealed until all tests have been made, when, if these have been satisfactory, it is released for shipment.

The samples are taken in cans, 9 in. high and 7½ in. in diameter. These cans are delivered in the preparation room where the contents are mixed and passed through a No. 20 sieve. Separate samples are then weighed out for mortar briquettes, for soundness pats, and for the specific-gravity and fineness tests. These are placed in smaller cans and a quantity sufficient for a re-test is held in the storage room awaiting the results of all the tests.

The sample for briquettes is mixed with three parts standard crushed quartz, and then taken to the briquette-making room, where eight briquettes are made, four for 7-day and four for 28-day tests. These are placed in the moist closet in damp air for 24 hours, then removed from the moulds, and placed in water for the remainder of the test period. At the proper time they are taken from the immersion tank and broken.

From the sample for soundness, four pats are made. The time of setting is determined on one of these pats. They are placed in the pan previously described, for 24 hours, then one is placed in running water and one in air for 28 days. The others are treated in the boiler, one in boiling water for 3 hours and one in steam at atmospheric pressure for 5 hours.

The sample taken for specific gravity and fineness is dried in the oven at 100° cent. in order to drive off moisture. Two samples are then carefully weighed out, 50 grammes for fineness and 64 grammes

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for specific gravity, and the determinations are made. As soon as anything unsatisfactory develops, a re-test is made. If, however, the cement satisfies all requirements, a report sheet containing all the data for a bin, is made out, and the cement is ready for shipment. From every fifth bin, special neat and mortar briquettes are made, which are intended for tests at ages up to ten years.

Salt-Water Laboratory.—The laboratory at Atlantic City, for conducting investigations into the effects of salt water on concrete and reinforced concrete, is situated so that water more than 25 ft. deep is available for immersion tests of the setting and deterioration of such materials.

Through the courtesy of the municipality of Atlantic City, Young's cottage, on old Young's Pier, has been turned over, at a nominal rental, to the Geological Survey for the conduct of these tests. The laboratory building is about 700 ft. from the boardwalk, and occupies a space about 100 by 45 ft. It is one story high, of frame-cottage construction, and stands on wooden piles at one side of the pier proper and about 20 ft. above the water, which is about 19 ft. deep at this point. Fresh running water, gas, electric light, and electric power are supplied to the building (Fig. 6).

In this laboratory investigations will be made of the cause of the failure and disintegration of cement and concrete subjected to the action of sea water. Tests are conducted so as to approach, as nearly as possible, the actual conditions found in concrete construction along the sea coast. All sea-water tests are made in the ocean, some will probably be paralleled by ocean-water laboratory tests and all by fresh-water comparative tests.

Cements, in the form of pats, briquettes, cubes, cylinders, and in a loose ground state, and also mortars and concretes in cube, cylinder, and slab form, are subjected to sea water.

The general plan for the investigations is as follows:

1.—Determination of the failing elements and the nature of the failure;

2.—Determination of the value of the theories advanced at the present time; and,

3.—Determination of a method of eliminating or chemically recombining the injurious elements.

Preliminary tests are in progress, including a study of the effect of salt water on mortars and concretes of various mixtures and ages.

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The proportions of these mixtures and the methods of mixing will be varied from time to time, as suggested by the progress of the tests.

Fire-Proofing Tests.—Tests of the fire-proofing and fire-resistive properties of various structural materials are carried on in the laboratories in Building No. 10, at Pittsburg, and in co-operation with the Board or Fire Underwriters at its Chicago laboratory (Fig. 2, Plate XIII). These tests include three essential classes of material: (*a*), clay products, protective coverings representative of numerous varieties of brick and fire-proofing tiles, including those on the market and those especially manufactured for these tests in the laboratory at Pittsburg; (*b*), characteristic granites of New England, with subsequent tests of the various building stones found throughout the United States; and (*c*), cement and concrete covering material, building blocks, and concrete reinforced by steel bars embedded at different depths for the purpose of studying the effect of expansion on the protective covering.

In co-operation with the physical laboratory, these tests include a study of the relative rates of conductivity of cement mortars and concretes. By embedding thermo-couples in cylinders composed of the materials under test, obtaining a given temperature by an electric coil, and noting the time required to raise the temperature at the various embedded couples to a given degree, the rate of conductivity may be determined. Other tests include those in muffles to determine the rate of expansion and the effect of heat and compressive stresses combined on the compressive strength of the various structural materials. The methods of making the panel tests, and the equipment used, are described and illustrated in Bulletin No. 329, and the results of the tests have been published in detail.*

Building Stones Investigations.—The field investigations of building stones are conducted by Mr. E. F. Burchard, and include the examination of the various deposits found throughout the United States. A study of the granites of New England has been commenced, which includes the collection of type specimens of fine, medium, and coarse-grained granites, and of dark, medium, and light-gray or white granites. A comparative series of these granites, consisting of prisms and cubes of 4 and 2 in., respectively, has been prepared.

* "Fire Resistive Properties of Various Building Materials," by Richard L. Humphrey, Bulletin No. 370, U. S. Geological Survey, 1909.

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PLAN OF LABORATORY FOR SALT-WATER TESTS AT ATLANTIC CITY, N.J.

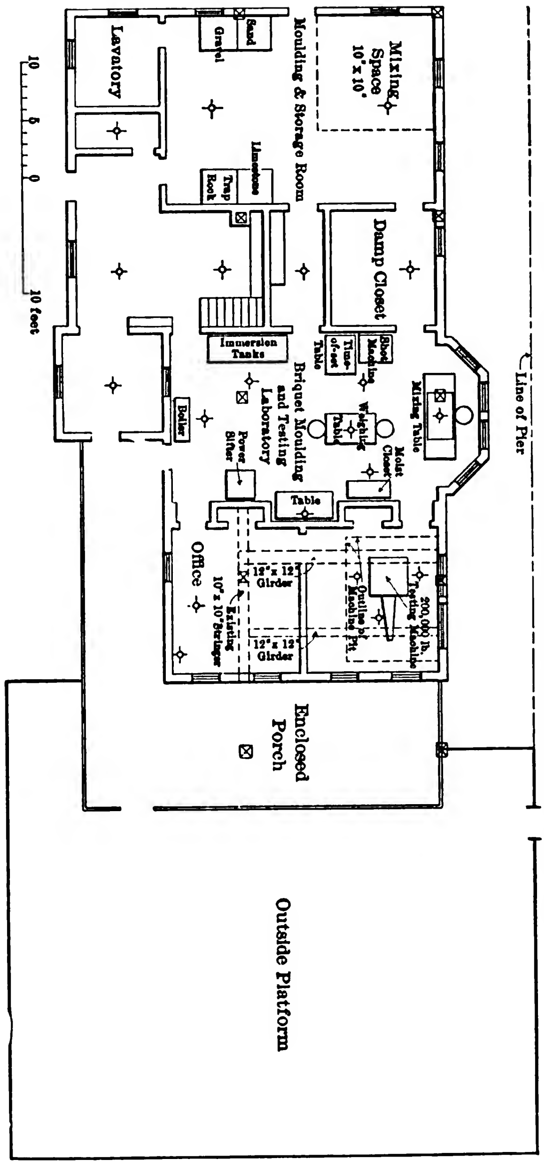

FIG. 6

The standard adopted for compressive test pieces in the 10 000 000-lb. machine is a prism, having a base of 12 in. and being 24 in. high. The tests include not only those for compression or crushing strength, but also those for resistance to compressive strains of the prisms and cubes, when raised to high temperatures in muffles or kilns; resistance to weathering, freezing, and thawing; porosity; fire-resisting qualities, etc.

In collecting field samples, special attention is paid to the occurrence of the stone, extent of the deposit, strike, dip, etc., and specimens are procured having their faces cut with reference to the bedding planes, in order that compressive and weathering tests may be made, not only in relation to these planes but at those angles thereto in which the material is most frequently used commercially. Attention is also paid to the results of blasting, in its relation to compressive strains, as blasting is believed to have a material effect on stones, especially on those which may occur in the foundations of great masonry dams, and type specimens of stone quarried by channeling, as well as by blasting, are collected and tested.

Clay and Clay Products Investigations.—These investigations are in charge of Mr. A. V. Bleininger, and include the study of the occurrence of clay beds in various parts of the United States, and the adaptability of each clay to the manufacture of the various clay products.

Experiments on grinding, drying, and burning the materials are conducted at the Pittsburg testing station, to ascertain the most favorable conditions for preparing and burning each clay, and to determine the most suitable economic use to which it may be put, such as the manufacture of building or paving bricks, architectural tiles, sewer tiles, etc.

The laboratory is equipped with various grinding and drying devices, muffles, kilns, and apparatus for chemical investigations, physical tests, and the manufacture and subsequent investigative tests of clay products.

This section occupies the east end of Building No. 10, and rooms on the first and second floors have been allotted for this work. In addition, a brick structure, 46 by 30 ft., provided with a 60-ft. iron stack, has been erected for housing the necessary kilns and furnaces.

On the ground floor of Building No. 10, adjoining the cement and

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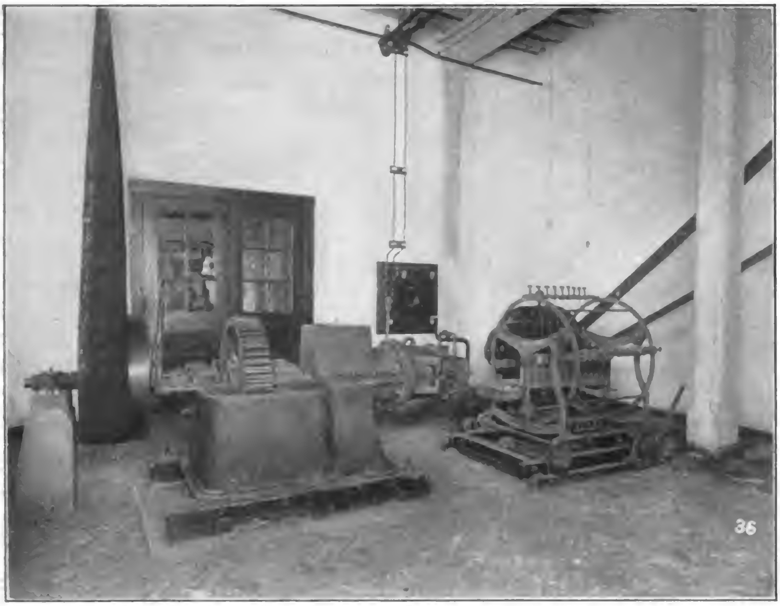

FIG. 1.—BRICK MACHINE AND UNIVERSAL CUTTER.

FIG. 2.—HOUSE-HEATING BOILERS, BUILDING NO. 21.

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concrete section, is a storage room for raw materials and products under investigation. Adjoining this room, and connecting with it by wide doors, is the grinding room, containing a 5-ft. wet pan, with 2 000-lb. rollers, to be used for both dry and wet grinding. Later, a heavy dry pan is to be installed. With these machines, even the hardest material can be easily disintegrated and prepared. In this room there is also a jaw crusher for reducing smaller quantities of very hard material, as well as a 30 by 16-in. iron ball mill, for fine grinding. These machines are belted to a line shaft along the wall across the building. Ample sink drainage is provided for flushing and cleaning the wet pan, when changing from one clay to another.

A large room adjoining is for the operation of all moulding and shaping machines, representing the usual commercial processes. At present these include an auger machine, with a rotary universal brick and tile cutter, Fig. 1, Plate XVI, and a set of brick and special dies, a hand repress for paving brick, and a hand screw press for dry pressing. The brick machine is operated from the main shaft which crosses the building in this room and is driven from a 50-h.p. motor. It is possible thus to study the power consumption under different loads and with different clays, as well as with varying degrees of water content in the clay. As the needs of the work demand it, other types of machines are to be installed. For special tests in which pressure is an important factor it is intended to fit up one of the compression testing machines of the cement section with the necessary dies, thus enabling the pressing to be carried on under known pressures. Crushing, transverse, and other tests of clay products are made on the testing machines of the cement and concrete laboratories.

Outside of the building, in a lean-to, there is a double-chamber rattler for the testing of paving brick according to the specifications of the National Brick Manufacturers' Association.

In the smaller room adjoining the machine laboratory there are two small wet-grinding ball mills, of two and four jars, respectively, and also a 9-leaf laboratory filter press.

The remaining room on the first floor is devoted to the drying of clays and clay wares. The equipment consists of a large sheet-iron drying oven of special construction, which permits of close regulation of the temperature (Fig. 7). It is heated by gas burners, and is used for the preliminary heat treatment of raw clays, in connection with

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the study of the drying problems of certain raw materials. It is intended to work with temperatures as high as 250° cent.

Another drying closet, heated by steam coils (Fig. 8), intended for drying various clay products, has been designed with special reference to the exact regulation of the temperature, humidity, and velocity of the air flowing through it. Both dryers connect by flues with an iron stack outside the building. This stack is provided with a suction fan, driven by a belt from an electric motor.

On the second floor are the chemical, physical, and research laboratories, dealing with the precise manipulations of the tests and investigations.

The chemical laboratory is fully equipped with the necessary apparatus for carrying on special chemical research in silicate chemistry, including electrical resistance furnaces, shaking devices, etc. It is not the intention to do routine work in this laboratory. The office adjoins this laboratory, and near it is the physical laboratory, devoted to the study of the structure of raw materials. The latter contains Nobel and Schoene elutriators, together with viscosimeters of the flow and the Coulomb and Clark electrical types, sieves, voluminometers, colorimeters, vernier shrinkage gauges, micrometers, microscopes, and the necessary balances.

The room across the hall is devoted to the study of the specific gravity, absorption, porosity, permeability, hardness, translucency, etc., of burnt-clay products, all the necessary apparatus being provided. In the two remaining rooms, intended for research work, special apparatus adapted to the particular investigation may be set up. All the rooms are piped for water, gas, compressed air, steam, and drainage, and wired for light and power.

In the kiln house there is a test kiln adapted for solid fuel and gas. It is of the down-draft type, with an available burning space of about 8 cu. ft. (Fig. 9). For heavier ware and the study of the fire behavior of clay products under conditions approaching those of practice, a round down-draft kiln, with an inside diameter of 6 ft., is installed. About 13 ft. above the floor level, and supported by iron beams, there is a flue parallel to the long side of the structure. This flue conducts the gases of the kilns to the stack, which is symmetrically located with reference to the kiln house. Natural gas is the principal fuel. In addition to these kilns, a small muffle furnace, fired with

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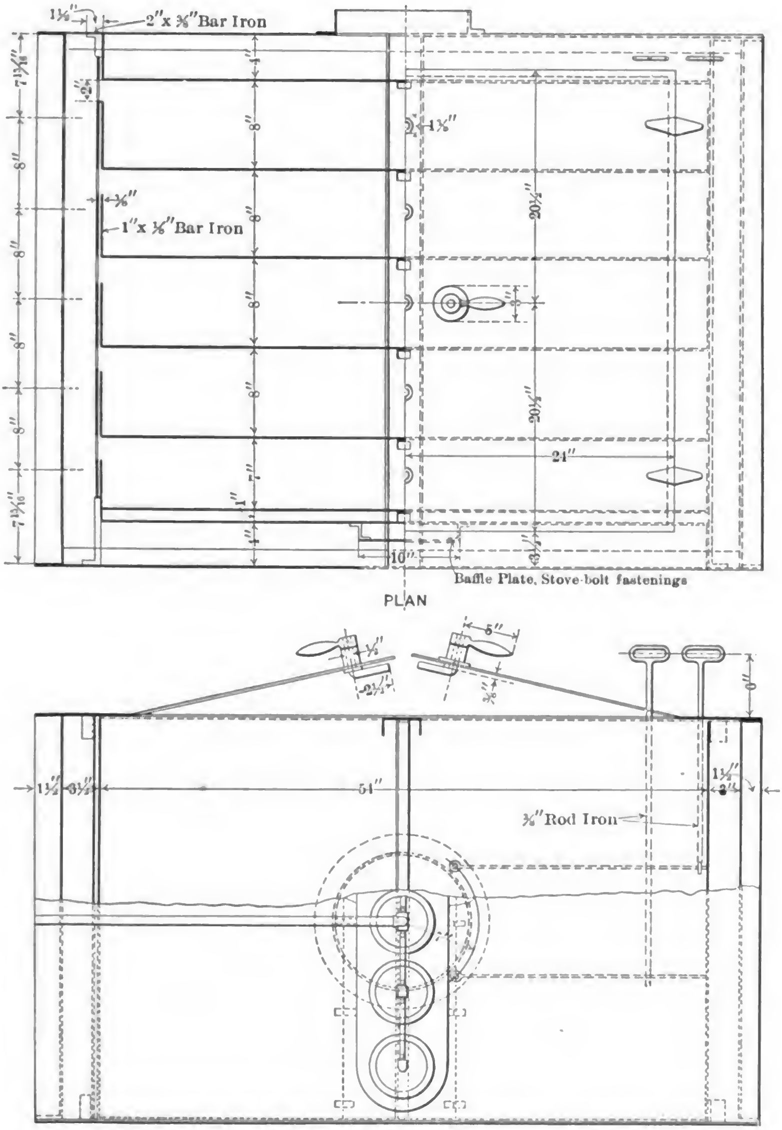

ELEVATION

CLAY-DRYING OVEN

12 0 12 Inches

FIG. 7.

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petroleum, is provided for the determination of melting points, and an electric carbon resistance furnace, with an aluminum muffle for high-temperature work. For crucible-fusion work, a gas-fired pot furnace is installed.

Along the north wall, bins are provided for the storage of fuel, clay, sand, and other kiln supplies. There are two floor drainage sinks, and electric current, steam, water, and compressed air, are provided.

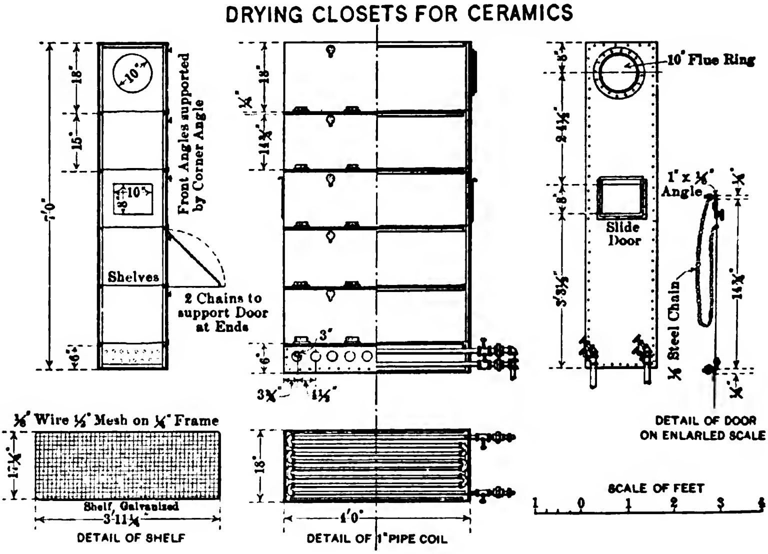

Fig. 8.

Results of the Work.—More than 39 300 separate test pieces have been made at the structural-materials testing laboratory. In connection with the study of these, 86 000 tests and nearly 14 000 chemical analyses have been made. Of these tests more than 13 600 have been of the constituent materials of concrete, including tensile tests of cement briquettes, compression tests of cylinders and cubes, and transverse tests of various specimens.

Nearly 1 200 beams of concrete or reinforced concrete, each 13 ft. long and 8 by 11 in. in cross-section, have been made, and, in connection with the investigation of the behavior of these beams, nearly 3 000 tests have been made. Nearly 900 of these beams, probably more than double the entire number made in other laboratories in

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the United States, during a period of more than 15 years, have been tested.

In the section of building blocks, 2 200 blocks have been tested, including, with auxiliary pieces, more than 4 500 tests; also, more than 900 pieces of concrete have been tested for permeability and shear. The physical tests have numbered 14 000; tests of steel for reinforcement, 3 800; and 550 tests to determine fire-resistive qualities of various building materials, have been made on 30 special panels, and on miscellaneous pieces.

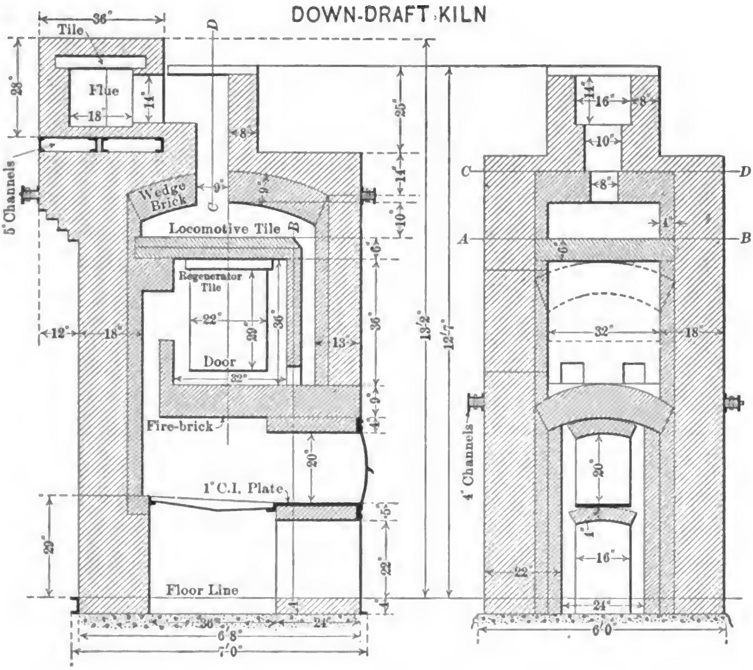

FIG. 9.

The tests of the permeability of cement mortars and concretes, and of water-proofing and damp-proofing materials, have numbered 3 470.

The results of the work of the Structural Materials Division have already appeared in preliminary bulletins, as follows: No. 324, "San Francisco Earthquake and Fire of April 18, 1906, and Their Effects on Structures and Structural Materials"; No. 329, "Organization, Equipment, and Operation of the Structural-Materials Testing Laboratories

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at St. Louis, Mo."; No. 331, "Portland Cement Mortars and Their Constituent Materials" (based on nearly 25 000 tests); No. 344, "Strength of Concrete Beams" (based on tests of 108 beams); No. 370, "Fire-Resistive Properties of Various Building Materials"; No. 387, "The Colloid Matter of Clay and its Measurements." A bulletin on the results of tests of reinforced concrete beams, one on the manufacture and chemistry of lime, and one on drying tests of brick, are in course of publication.

Fuel Investigations.

The scope of the fuel investigations has been planned to conform to the provisions of the Act of Congress which provides for analyzing and testing coals, lignites, and other mineral fuel substances belonging to the United States, or for the use of the United States Government, and examinations for the purpose of increasing the general efficiency or available supply of the fuel resources in the United States.

In conformity with this plan, the investigations inaugurated at St. Louis had for their initial object the analyzing and testing of the coals of the United States, using in this work samples of from 1 to 3 carloads, collected with great care from typical localities in the more important coal fields of the country, with a view to determining the relative values of those different fuels. In the work at Norfolk, during 1907, this purpose was modified to the extent of keeping in view relative fuel efficiencies for naval purposes. The tests at Denver have been on coal from Government land or from land contiguous thereto, and are conducted solely with a view to perfecting methods of coking this coal by prior washing and by manipulation in the process of coking.

Three general lines of inquiry are embodied in the plan of investigation undertaken and contemplated by the Technologic Branch, after conference and with the advice and approval of the Advisory Board: 1. The ascertainment of the best mode of utilizing any fuel deposit owned or to be used by the Government, or the fuel of any extensive deposit as a whole, by conducting a more thorough investigation into its combustion under steam boilers, conversion into producer gas, or into coke, briquettes, etc. 2. The prevention of waste, through the study of the possibility of improvement in the methods of mining, shipping, utilizing, etc. 3. The inspection and analysis of coal and

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lignite purchased under specification for the use of the Government, to ascertain its heating value, ash, contained moisture, etc.

The first general line of work concerns the investigation and testing of the fuel resources of the United States, and especially those belonging to the Federal Government, to determine a more efficient and more economical method of utilizing the same. This work has developed along the following lines:

The collection of representative samples for chemical analysis, and calorimeter tests by a corps of skilled mine samplers, from the mines selected as typical of extensive deposits of coal in a given field or from a given bed of coal; and the collection from the same mines of larger samples of from 1 to 3 carloads, shipped to the testing station for tests in boiler furnaces, gas producers, etc., as a check on the analysis and calorimeter tests;

The testing of each coal received to determine the most efficient and least wasteful method of use in different furnaces suitable for public buildings or power plants or ships of the Government;

The testing of other portions of the same shipment of coal in the gas producer, for continuous runs during periods of a few days up to several weeks, in order to determine the availability of this fuel for use in such producers, and the best method of handling it, to determine the conditions requisite to produce the largest amount of high-grade gas available for power purposes;

The testing of another portion of the same coal in a briquette machine at different pressures and with different percentages and kinds of binder, in order to determine the feasibility of briquetting the slack or fine coal. Combustion tests are then made of these briquettes, to determine the conditions under which they may be burned advantageously;

Demonstrations, on a commercial scale, of the possibility of producing briquettes from American lignites, and the relative value of these for purposes of combustion as compared with the run-of-mine coal from which the briquettes are made;

The finding of cheaper binders for use in briquetting friable coals not suited for coking purposes;

Investigations into the distribution, chemical composition, and calorific value of the peat deposits available in those portions of the United States where coal is not found, and the preparation of such

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peat for combustion, by drying or briquetting, to render it useful as a local substitute for coal;

Investigations into the character of the various petroleums found throughout the United States, with a view to determining their calorific value, chemical composition, and the various methods whereby they may be made most economically available for more efficient use as power producers, through the various methods of combustion;

Investigations and tests into the relative efficiency, as power producers in internal-combustion engines, of the heavier distillates of petroleum, as well as of kerosene and gasoline, in order to ascertain the commercial value and relative efficiency of each product in the various types of engines;

Investigations into the most efficient methods of utilizing the various coals available throughout the United States for heating small public buildings, army posts, etc., in order that these coals may be used more economically than at present;

Investigative studies into the processes of combustion within boiler furnaces and gas producers to ascertain the temperatures at which the most complete combustion of the gases takes place, and the means whereby such temperatures may be produced and maintained, thus diminishing the loss of values up the smokestack and the amount of smoke produced;

Investigations and tests into the possibilities of coking coals which have hitherto been classed as non-coking, and the making of comparative tests of all coals found in the United States, especially those from the public lands of the West;

Investigations, by means of washing in suitable machines, to determine the possibility of improving the quality of American coals for various methods of combustion, and with a view to making them more available for the production of coke of high-grade metallurgical value, as free as possible from sulphur and other injurious substances.

At each stage of the process of testing, samples of the coal have been forwarded to the chemical laboratory for analyses; combustion temperatures have been measured; and samples of gas collected from various parts of the combustion chambers of the gas producers and boiler furnaces have been analyzed, in order that a study of these data may throw such light on the processes of combustion and indicate such necessary changes in the apparatus, as might result in larger economies in the use of coal.

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The second line of investigation concerns the methods of mining and preparing coal for the market, and the collection of mine samples of coal, oil, etc., for analysis and testing. It is well known that, under present methods of mining, from 10 to 75% of any given deposit of coal is left underground as props and supports, or as low-grade material, or in overlying beds broken up through mining the lower bed first. An average of 50% of the coal is thus wasted or rendered valueless, as it cannot be removed subsequently because of the caving or falling in of the roofs of abandoned galleries and the breaking up of the adjoining overlying beds, including coal, floor, and roof.

The investigations into waste in mining and the testing of the waste, bone, and slack coal in gas producers, as briquettes, etc., have, for their purpose, the prevention of this form of waste by demonstrating that these materials, now wasted, may be used profitably, by means of gas producers and engines, for power purposes.

The third general line of investigation concerns the inspection and sampling of fuel delivered to the Government under purchase contracts, and the analyzing and testing of the samples collected, to determine their heating value and the extent to which they may or may not comply with the specifications under which they are purchased. The coal delivered at the public buildings in the District of Columbia is sampled by special representatives of the Technologic Branch of the Survey. The taking of similar samples at public buildings and posts throughout the United States, and the shipment of the samples in hermetically sealed cans or jars to the chemical laboratory at Washington, is for the most part looked after by special officers or employees at each place. These purchases are made, to an increasing extent, under specifications which provide premiums for coal delivered in excess of standards, and penalties for deliveries below standards fixed in the specifications. The standard for bituminous coals is based mainly on the heat units, ash, and sulphur, while that for anthracite coal is based mainly on the percentage of ash and the heat units.

In connection with all these lines of fuel testing, certain research work, both chemical and physical, is carried on to determine the true composition and properties of the different varieties of coal, the changes in the transformation from peat to lignite, from lignite to bituminous coal, and from bituminous to anthracite coal, and the chemical and physical processes in combustion. Experiments are conducted concerning the destructive distillation of fuels; the by-products of coking

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processes; the spontaneous combustion of coal; the storage of coal, and the loss in value in various methods of storing; and kindred questions, such as the weathering of coal. These experiments may yield valuable results through careful chemical research work supplemented by equally careful observations in the field.

Inspection and Mine Sampling.—In the Geological Survey Building, at Washington, coal purchased for Government use on a guaranteed-analysis or heat-value basis, is inspected and sampled.

Some of the employees on this work are constantly at the mines taking samples, or at public works inspecting coal for Government use, while others are stationed at Washington to look after the deliveries of coal to the many public buildings, and to collect and prepare samples

COAL-SAMPLING ROOM, GEOLOGICAL SURVEY, WASHINGTON, D.C.

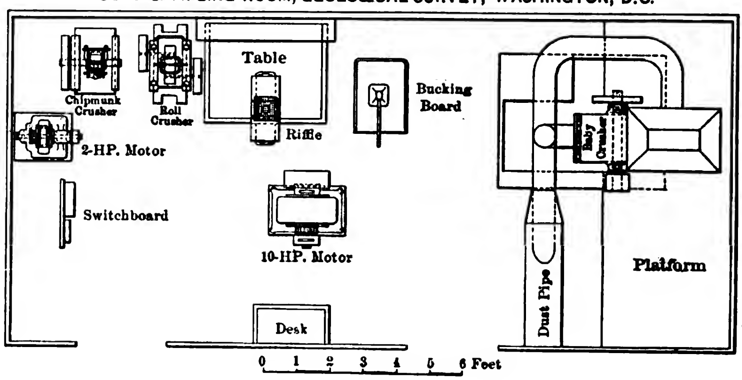

FIG. 10.

taken from these deliveries for analysis, as well as to prepare samples received from public works and buildings in other parts of the country.

The preparation of these samples is carried on in a room in the basement of the building, where special machinery has been installed for this work. Fig. 10 shows a plan of this room and the arrangement of the sampling and crushing machinery.

The crushing of the coal produces great quantities of objectionable dust, and to prevent this dust from giving trouble outside the sampling room, the wooden partitions on three sides of the room (the fourth side being a masonry wall) are completely covered on the outside with galvanized sheet iron. The only openings to the room are two doors, which are likewise covered with sheet iron, and provided with broad

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flanges of the same material, in order to seal effectually the openings when the doors are shut. Fresh air is drawn into the room by a fan, through a pipe leading to the outer air. A dust-collecting system which carries the coal dust and spent air from the room, consists of an arrangement of 8-in. and 12-in. pipes leading from hoods, placed over the crushing machines, to the main furnace stack of the building. The draft in this stack draws all the dust from the crushers directly through the hoods to the main pipe, where most of it is deposited.

The equipment of the sampling room consists of one motor-driven, baby hammer crusher, which has a capacity of about 1 ton per hour and crushes to a fineness of ¼-in. mesh; one adjustable chipmunk jaw crusher, for 5- and 10-lb. samples; one set of 4½ by 7½-in. rolls, crushing to 60 mesh, for small samples; one large bucking board, and several different sizes of riffle samplers for reducing samples to small quantities. The small crushers are belted to a shaft driven by a separate motor from that driving the baby crusher.

In conducting the inspection of departmental purchases of coal in Washington, the office is notified whenever a delivery of coal is to be made at one of the buildings, and an inspector is sent, who remains during the unloading of the coal. He is provided with galvanized-iron buckets having lids and locks; each bucket holds about 60 lb. of coal. In these buckets he puts small quantities of the coal taken from every portion of the delivery, and when the delivery has been completed, he locks the buckets and notifies the office to send a wagon for them. The buckets are numbered consecutively, and the inspector makes a record of these numbers, the date, point of delivery, quality of coal delivered, etc. The buckets are also tagged to prevent error. He then reports to the office in person, or by telephone, for assignment to another point in the city. All the samples are delivered to the crushing room in the basement of the Survey Building, to be prepared for analysis.*

Samples taken from coal delivered to points outside of Washington are taken by representatives of the department for which the coal is being purchased, according to instructions furnished them, and, from time to time, the regular inspectors are sent to see that these instructions are being complied with. These samples are crushed by hand, reduced to about 2 lb. at the point where they are taken, and sent

* "Purchasing Coal Under Government Specifications," by J. S. Burrows, Bulletin No. 378, U. S Geological Survey, 1909.

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to Washington, in proper air-tight containers, by mail or express, accompanied by appropriate descriptions.

Each sample is entered in the sample record book when received, and is given a serial number. For each contract a card is provided giving information relative to the contract. On this card is also entered the serial number of each sample of coal delivered under that contract.

After the samples are recorded, they are sent to the crushing room, where they are reduced to the proper bulk and fineness for analysis. They are then sent, in rubber-stoppered bottles, accompanied by blank analysis report cards and card receipts, one for each sample, showing the serial numbers, to the fuel laboratory for analysis. The receipt card for each sample is signed and returned to the inspection office, and when the analysis has been made, the analysis report card showing the result is returned. This result is entered at once on the contract card, and when all analyses have been received, covering the entire delivery of coal, the average quality is calculated, and the results are reported to the proper department.

The matter of supplying the Pittsburg plant with fuel for test purposes is also carried on from the Washington office. Preliminary to a series of investigations, the kinds and amounts of coal required are decided on, and the localities from which these coals are to be obtained are determined. Negotiations are then opened with the mine owners, who, in most cases, generously donate the coal. When the preliminaries have been arranged, an inspector is sent to the mine to supervise the loading and shipment of the coal. This inspector enters the mine and takes, for chemical analysis, small mine samples which are sent to the laboratory at Pittsburg in metal cans by mail, accompanied by proper identification cards. The results of the analysis are furnished to the experts in charge at the testing plant, for their information and guidance in the investigations for which the coal was shipped.

All samples for testing purposes are designated consecutively in the order of shipment, "Pittsburg No. 1," "Pittsburg No. 2," etc. A complete record of all shipments is kept on card forms at the Pittsburg plant, and a duplicate set of these is on file in the inspection office at Washington.

Analysis of Fuels.—The routine analyses of fuel used in the combustion tests at Pittsburg, and of the gases resulting from combustion

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or from explosions in the testing galleries, or sampled in the mines, are made in Building No. 21.* A small laboratory is also maintained on the second floor of the south end of Building No. 13, for analyses of gases resulting from combustion in the producer-gas plant, and from explosions in Galleries Nos. 1 and 2, etc. From four to six chemists are continually employed in this laboratory (in 8-hour shifts), during prolonged gas-producer tests, and three chemists are also employed in analyzing gases relating to mine explosions.

In addition to these gas analyses, there are also made in the main laboratory, analyses and calorific tests of all coal samples collected by the Geological Survey in connection with its land-classification work on the coal lands of the Western States. Routine analyses of mine, car, and furnace samples of fuels for testing, before and after washing and briquetting, before coking and the resultant coke, and extraction analyses of binders for briquettes, etc., are also made in this laboratory.

The fuel-testing laboratory at Washington is equipped with three Mahler bomb calorimeters and the necessary balances and chemical equipment required in the proximate analysis of coal. More than 650 deliveries of coal are sampled each month for tests, representing 50 000 tons purchased per month, besides daily deliveries, on shipboard, of 550 000 tons of coal for the Panama Railroad. The data obtained by these tests furnish the basis for payment. The tests cover deliveries of coal to the forty odd bureaus, and to the District Municipal buildings in Washington; to the arsenals at Watertown, Mass., Frankford, Pa., and Rock Island, Ill.; and to a number of navy yards, through the Bureau of Yards and Docks; to military posts in various parts of the country; for the Quartermaster-General's Department; to the Reclamation Service; to Indian Agencies and Soldiers' Homes; to several lighthouse districts; and to the superintendents of the various public buildings throughout the United States, through the Treasury Department; etc. During 1909, the average rate of reporting fuel samples was 540 per month, requiring, on an average, six determinations per sample, or about 3 240 determinations per month.

Fuel-Research Laboratories.—Smaller laboratories, occupying, on the average, three rooms each, are located in Building No. 21. One is used for chemical investigations and calorific tests of petroleum collected from the various oil fields of the United States; another is used

* "Experimental Work in the Chemical Laboratory," by N. W. Lord, Bulletin No. 323, U. S. Geological Survey, 1907; "Operations of the Coal Testing Plant, St. Louis, Mo." Professional Paper No. 48, U. S. Geological Survey, 1906.

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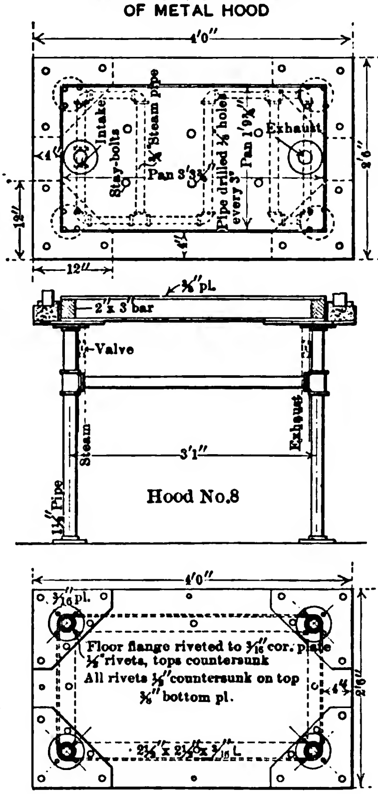
for investigations relative to the extraction of coal and the rapidity of oxidization of coals by standard solutions of oxidizing agents; and another is occupied with investigations into the destructive distillation of coal. The researches under way show the wide variation in chemical composition and calorific value of the various crude oils, indicate the possibility of the extraction of coal constituents by solvents, and point to important results relative to the equilibrium of gases at high temperatures in furnaces and gas producers. The investigations also bear directly on the coking processes, especially the by-product process, as showing the varying proportion of each of the volatile products derivable from types of coals occurring in the various coal fields of the United States, the time and temperature at which these distillates are given off, the variation in quality and quantity of the products, according to the conditions of temperature, and, in addition, explain the deterioration of coals in storage, etc.

At the Washington office, microscopic investigations into the life history of coal, lignite, and peat are being conducted. These investigations have already progressed far enough to admit of the identification of some of the botanical constituents of the older peats and the younger lignites, and it is believed that the origin of the older lignites, and even of some of the more recent bituminous coals, may be developed through this examination.

PLAN OF CONSTRUCTION DETAILS OF METAL HOOD



Bottom of table

FIG. 11.

In the chemical laboratories, in Building No. 21, the hoods (Figs. 11 and 12) are of iron, with a brick pan underneath. They are supported on iron pipes, as are most of the other fixtures in the laboratories

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ELEVATION OF CONSTRUCTION DETAILS OF METAL HOOD

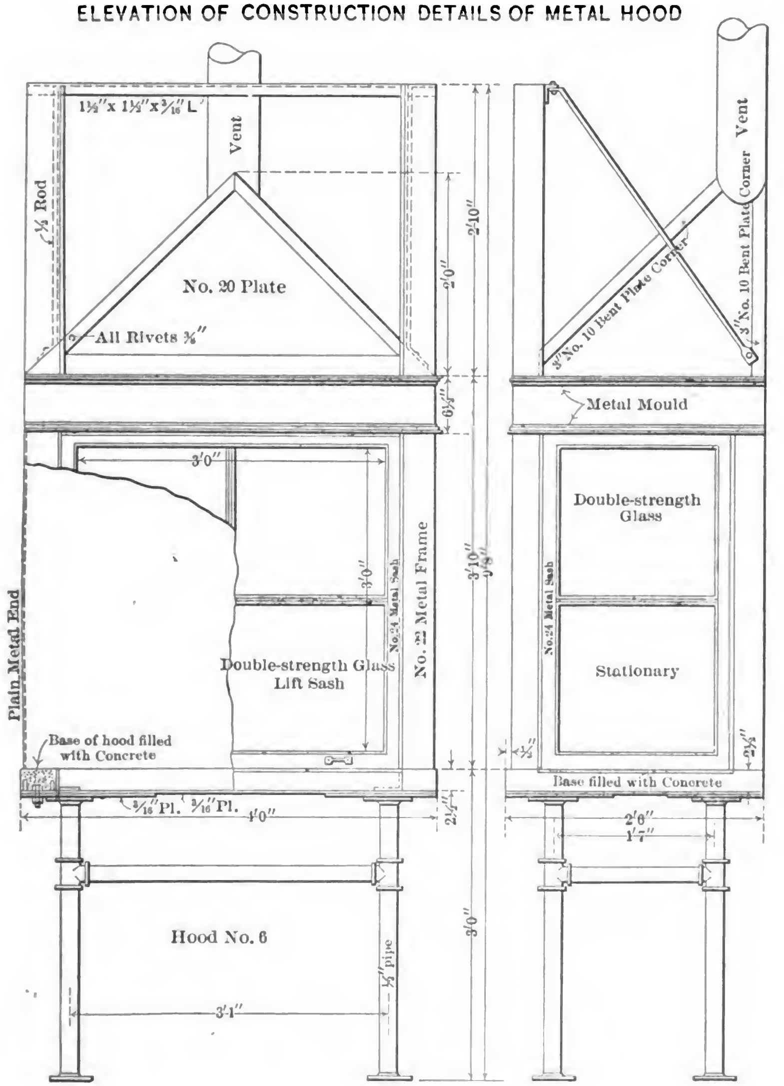

FIG. 12.

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in this building. The hood proper is of japanned, pressed-iron plate, No. 22 gauge, the same material being used for the boxes, slides, and bottom surrounding the hood. The sash is hung on red copper pulleys, and the corners of the hood are reinforced with pressed, japanned, riveted plate to which the ventilating pipe is riveted.

There is some variety in the cupboards and tables provided in the various laboratories, but, in general, they follow the design shown in Fig. 13. The table tops, 12 ft. long, are of clear maple in full-length pieces, ⅞ in. thick and 2⅝ in. wide, laid on edge and drilled at 18-in. intervals for bolts. These pieces are glued and drawn together by the bolts, the heads of which are countersunk. The tops, planed off, sanded, and rounded, are supported on pipe legs and frames of 1¼ by 1½-in. galvanized-iron pipe with screw flanges fitting to the floor and top. Under the tops are drawers and above them re-agent shelves. Half-way between the table top and the floor is a wire shelf of a frame-work of No. 2 wire interlaced with No. 12 weave of ⅝-in. square mesh.

Certain of the tables used in the laboratory are fitted with cup-boards beneath and with drawers, and, in place of re-agent stands, porcelain-lined sinks are sunk into them. These tables follow, in general style and construction, the re-agent tables. The tables used in connection with calorimeter determinations are illustrated in Fig. 14. The sinks provided throughout these laboratories are of standard porce-lain enamel, rolled rim, 18 by 13 in., with enameled back, over a sink and drain board, 24 in. long on the left side, though there are varia-tions from this type in some instances.

The plumbing includes separate lines of pipe to each hood and table; one each for cold water, steam at from 5 to 10 lb. pressure, com-pressed air, natural gas, and, in some cases, live steam at a pressure of 60 lb.

On each table is an exposed drainage system of 2½-in. galvanized-iron pipe, in the upper surface of which holes have been bored, through which the various apparatus drain by means of flexible connections of glass or rubber. These pipes and the sinks, etc., discharge into main drains, hung to the ceiling of the floor beneath. These drains are of wood, asphaltum coated, with an inside diameter ranging from 3 to 6 in., and at the proper grades to secure free discharge. These wooden drain-pipes are made in short lengths, strengthened by a spiral wrapping of metal bands, and are tested to a pressure of 40 lb. per sq. in. Angles are turned and branches connected in 4- and 6-in. square headers.

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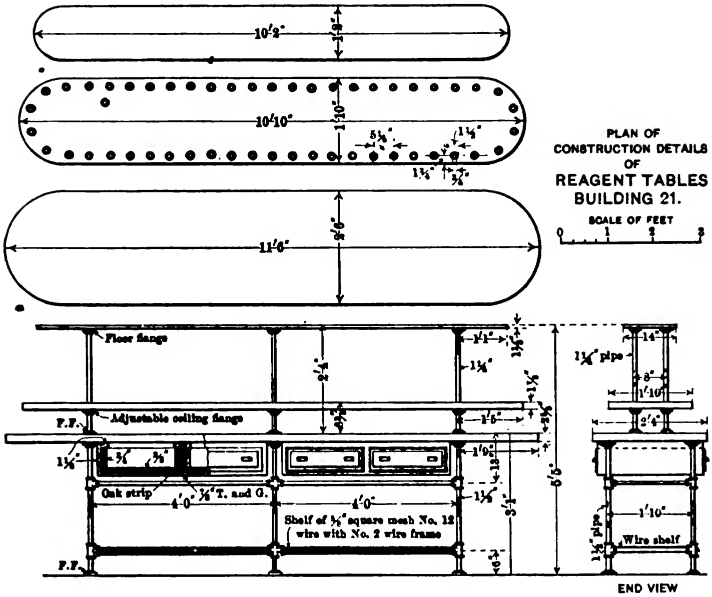

FIG. 13.

CONSTRUCTION DETAILS OF CALORIMETER TABLES

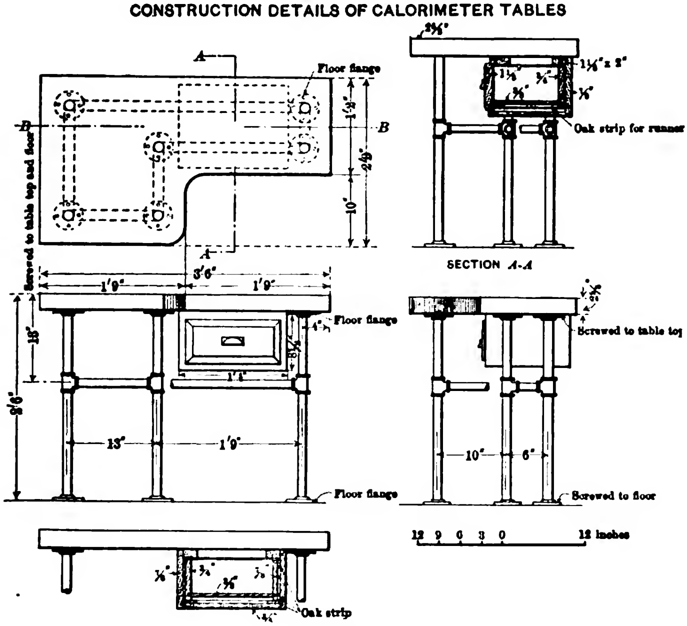

FIG. 14.

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The entire building is ventilated by a force or blower fan in the basement, and by an exhaust fan in the attic with sufficient capacity to insure complete renewal of air in each laboratory once in 20 min.

The blower fan is placed in the center of the building, on the ground floor, and is 100 in. in diameter. Its capacity is about 30 000 cu. ft. of air per min., and it forces the air, through a series of pipes, into registers placed in each of the laboratories.

The exhaust fan, in the center of the attic, is run at 550 rev. per min., and has a capacity of 22 600 cu. ft. of air per min. It draws the air from each of the rooms below, as well as from the hoods, through a main pipe, 48 in. in diameter.

Steaming and Combustion Tests.—The investigations included under the term, fuel efficiency, relate to the utilization of the various types of fuels found in the coal and oil fields, and deal primarily with the combustion of such fuels in gas producers, in the furnaces of steam boilers, in locomotives, etc., and with the efficiency and utilization of petroleum, kerosene, gasoline, etc., in internal-combustion engines. This work is under the general direction of Mr. R. L. Fernald, and is conducted principally in Buildings Nos. 13 (Plate XVII) and 21.

For tests of combustion of fuels purchased by the Government, the equipment consists of two Heine, water-tube boilers, each of 210 h.p., set in Building No. 13. One of these boilers is equipped with a Jones underfeed stoker, and is baffled in the regular way. At four points in the setting, large pipes have been built into the brick wall, to permit making observations on the temperature of the gas, and to take samples of the gas for chemical analysis.

The other boiler is set with a plain hand-fired grate. It is baffled to give an extra passage for the gases (Fig. 15). Through the side of this boiler, at the rear end, the gases from the long combustion chamber (Plate XVIII) enter and take the same course as those from the hand-fired grate. Both the hand-fired grate and the long combustion chamber may be operated at the same time, but it is expected that usually only one will be in operation. A forced-draft fan has been installed at one side of the hand-fired boiler, to provide air pressure when coal is being burned at high capacity. This fan is also connected in such a way as to furnish air for the long combustion chamber when desired. A more complete description of the boilers may be found in Professional Paper No. 48, and Bulletin No. 325 of

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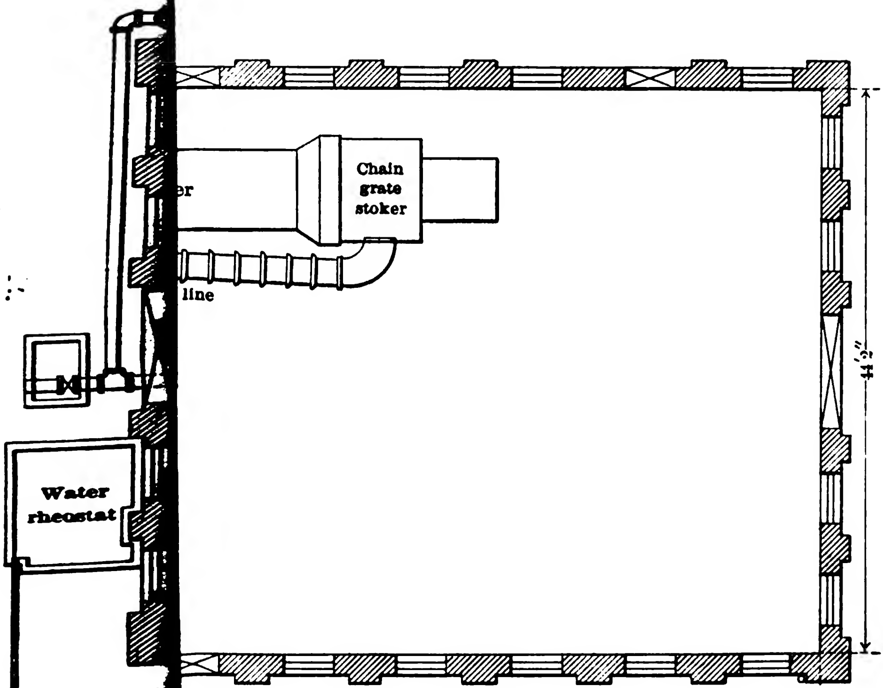
PLATE XVII.
TRANS. AM. SOC. CIV. ENGRS.
VOL. LXX, No. 1171.
WILSON ON
FEDERAL INVESTIGATIONS OF MINE ACCIDENTS, ETC.

PLAN OF BUILDING 13,
TESTING STATION AT PITTSBURG, PA.



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the U. S. Geological Survey, in which the water-measuring apparatus is also described.*

On account of the distance from Building No. 21 to the main group of buildings, it was considered inadvisable to attempt to furnish steam from Building No. 13 to Building No. 21, either for heating or power purposes. In view, moreover, of the necessity of installing various types and sizes of house-heating boilers, on account of tests to be made thereon in connection with these investigations, it was decided to install these boilers in the lower floor of Building

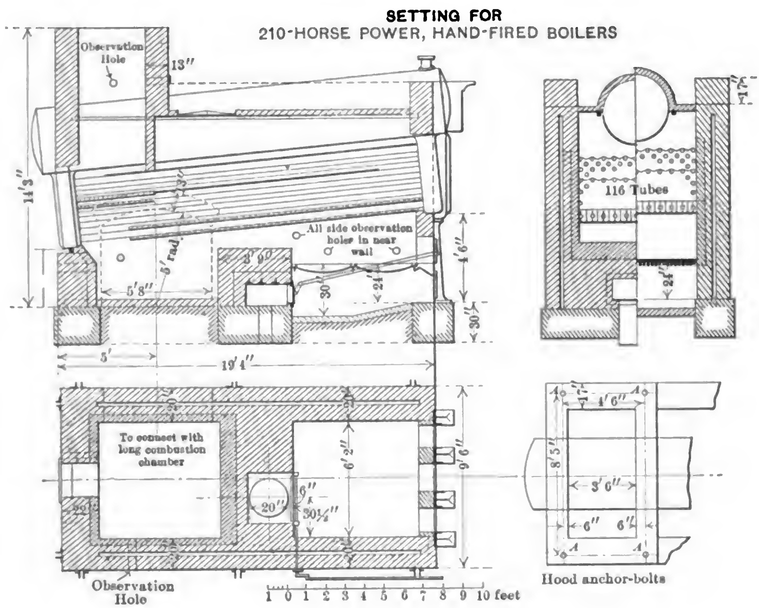

FIG. 15.

No. 21, where they could be utilized, not only in making the necessary tests, but in furnishing heat and steam for the building and the chemical laboratories therein.

In addition to the physical laboratory on the lower floor of Building No. 21, and the house-heating boiler plant with the necessary coal storage, there are rooms devoted to the storage of heavy supplies, samples of fuels and oils, and miscellaneous commercial apparatus. One room is occupied by the ventilating fan and one is used for the

* Also Bulletins Nos. 290, 332, 334, 363, 366, 367, 373, 402, 403, and 412, U. S. Geological Survey.

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necessary crushers, rolls, sizing screens, etc., required in connection with the sampling of coal prior to analysis.

The Quartermaster's Department having expressed a wish that tests be made of the heating value and efficiency of the various fuels offered that Department, in connection with the heating of military posts throughout the country, three house-heating boilers were procured which represent, in a general way, the types and sizes used in a medium-sized hospital or other similar building, and in smaller residences (Fig. 2, Plate XVI). The larger apparatus is a horizontal return-tubular boiler, 60 in. in diameter, 16 ft. long, and having fifty-four 4-in. tubes.*

In order to determine whether such a boiler may be operated under heating conditions without making smoke, when burning various kinds of coal, it has been installed in accordance with accepted ideas regarding the prevention of smoke. A fire-brick arch extends over the entire grate surface and past the bridge wall. A baffle wall has been built in the combustion chamber, which compels the gases to pass downward and to divide through two openings before they reach the boiler shell. Provision has been made for the admission of air at the front of the furnace, underneath the arch, and at the rear end of the bridge wall, thus furnishing air both above and below the fire. It is not expected that all coals can be burned without smoke in this furnace, but it is desirable to determine under what conditions some kinds of coals may be burned without objectionable smoke.†

For sampling the gases in the smokebox of the horizontal return-tubular boiler, a special flue-gas sampler was designed, in order to obtain a composite sample of the gases escaping from the boiler.

The other heaters are two cast-iron house-heating boilers. One can supply 400 sq. ft. of radiation and the other about 4 000 sq. ft. They were installed primarily for the purpose of testing coals to determine their relative value when burned for heating purposes. They are piped to a specially designed separator, and from this to a pressure-reducing valve. Beyond this valve an orifice allows the steam to escape into the regular heating mains. This arrangement makes it possible to maintain a practically constant load on the boilers.

* "Tests of Coal for House-Heating Boilers," by D. T. Randall, Bulletin No. 336, U. S. Geological Survey, 1908.

† "The Smokeless Combustion of Coal," by D. T. Randall and H. W. Weeks, Bulletin No. 373, U. S. Geological Survey, 1909.

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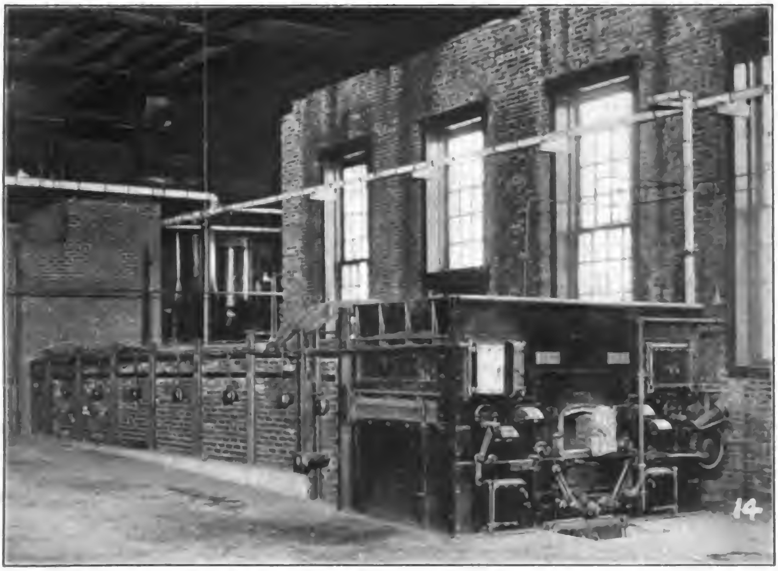

FIG. 1.—LONG COMBUSTION CHAMBER.

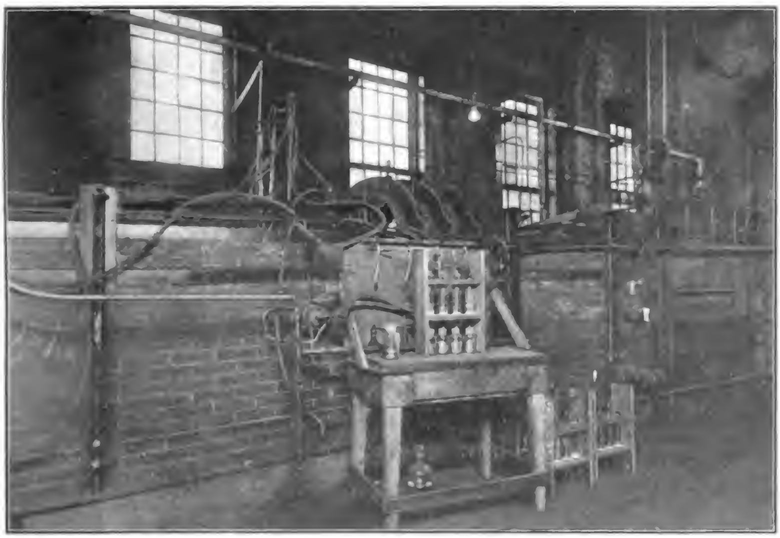

FIG. 2.—GAS SAMPLING APPARATUS, LONG COMBUSTION CHAMBER.

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There is a fourth boiler, designed and built for testing purposes by the Quartermaster's Department. This is a tubular boiler designed on the lines of a house-heating boiler, but for use as a calorimeter to determine the relative heat value of different fuels reduced to the basis of a standard cord of oak wood.

A series of research tests on the processes of combustion is being conducted in Building No. 13, by Mr. Henry Kreisinger. These tests are being made chiefly in a long combustion chamber (Figs. 16 and 17, and Figs. 1 and 2, Plate XVIII), which is fed with coal from a Murphy mechanical stoker, and discharges the hot gases at the rear end of the combustion chamber, into the hand-fired Heine boiler. The walls and roof of this chamber are double; the inner wall is 9 in.

CROSS-SECTIONS OF CHAMBER AND OF FURNACE, LONG COMBUSTION CHAMBER

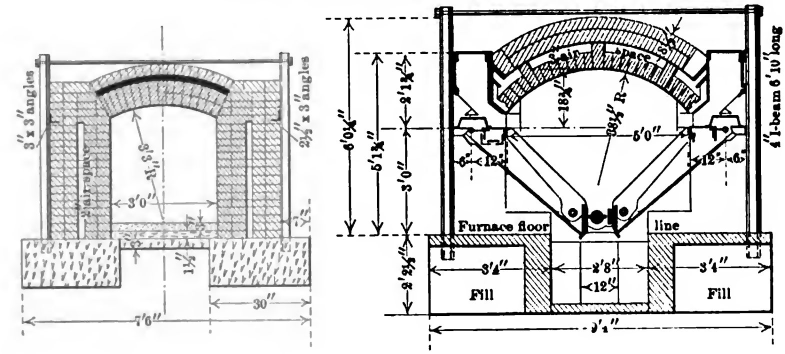

FIG. 16.

thick, of fire-brick; the outer one is 8 in. thick, and is faced with red pressed brick. Between the walls of the sides there is a 2-in. air space, and between them on the roof a 1-in. layer of asbestos paste is placed. The inner walls and roof have three special slip-joints, to allow for expansion. The floor is of concrete, protected by a 1½-in. layer of asbestos board, which in turn is covered by a 3-in. layer of earth; on top of this earth there is a 4-in. layer of fire-brick (not shown in the drawings).

Inasmuch as one of the first problems to be attacked will be the determination of the length of travel and the time required to complete combustion in a flame in which the lines of stream flow are nearly parallel, great care was taken to make the inner surfaces of the

tunnel smooth, and all corners and hollows are rounded out in the direction of travel of the gases.

Provision is made, by large peep-holes in the sides, and by smaller sampling holes in the top, for observing the fuel bed at several points and also the flame at 5-ft. intervals along the tunnel. Temperatures and gas samples are taken simultaneously at a number of points through these holes, so as to determine, if possible, the progress of combustion (Fig. 1, Pate XVIII).

About twenty thermo-couples are embedded in the walls, roof, and floor, some within 1 in. of the inside edge of the tunnel walls, and some in the red pressed brick near the outer surface, the object of which is to procure data on heat conduction through well-built brick walls* (Fig. 2, Plate XVIII).

In order to minimize the leakage of air through the brickwork, the furnace and tunnel are kept as nearly as possible at atmospheric pressure by the combined use of pressure and exhausting fans. Nevertheless, the leakage is determined periodically as accurately as possible.

At first a number of tests were run to calibrate the apparatus as a whole, all these preliminary tests being made on cheap, carefully inspected, uniform screenings from the same seam of the same mine near Pittsburg. Later tests will be run with other coals of various volatile contents and various distillation properties.

It is anticipated that the progress of the tests may suggest changes in the construction or operation of this chamber. It is especially contemplated that the section of the chamber may be narrowed down by laying sand in the bottom and fire-brick thereon; also that baffle walls may be built into various portions of it, and that cooling surfaces with baffling may be introduced. In addition to variations in the tests, due to changes in construction in the combustion chamber, there will be variations in the fuels tested. Especial effort will be made to procure fuels ranging in volatile content from 15 to 27 and to 40%, and those high in tar and heavy hydro-carbons. It is also proposed to vary the conditions of testing by burning at high rates, such as at 15, 20, and 30 lb. per ft. of grate surface, and even higher. Records will be kept of the weight of coal fired and of each firing, of

* "The Flow of Heat through Furnace Walls," by W. T. Ray and H. Kreisinger. Bulletin (in press), U. S. Geological Survey.

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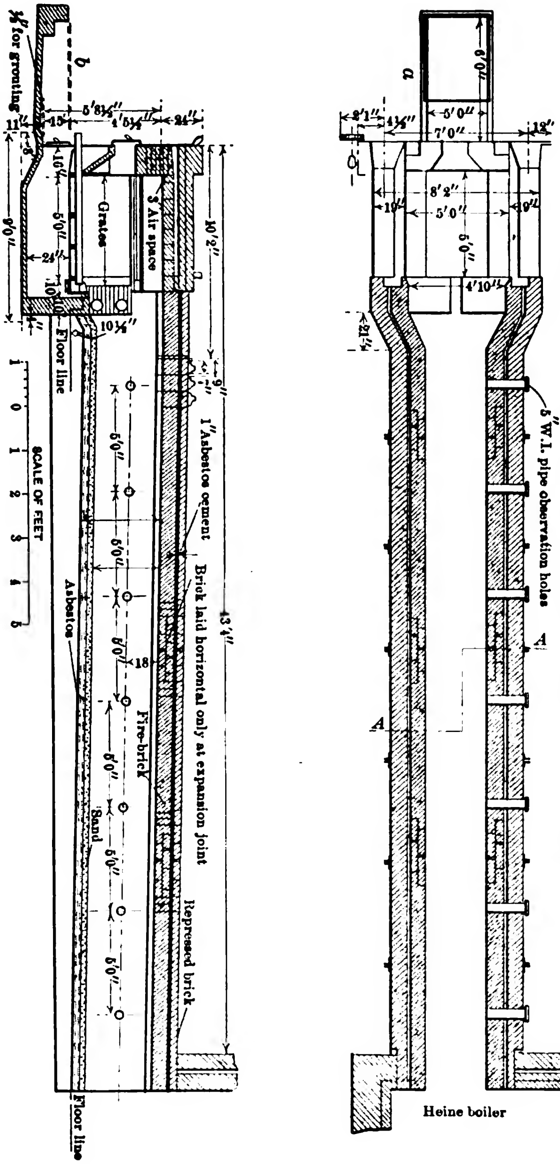

LONGITUDINAL SECTIONS OF LONG COMBUSTION CHAMBER

FIG. 17.

the weight of ash, etc.; samples of coal and of ash will be taken for chemical and physical analysis, as well as samples of the gas, and other essential data. These records will be studied in detail.

A series of heat-transmission tests undertaken two years ago, is being continued on the ground floor of Building No. 21, on modified apparatus reconstructed in the light of the earlier experiments by Mr. W. T. Ray. The purpose of the tests on this apparatus has been to determine some of the laws controlling the rate of transmission of heat from a hot gas to a liquid and *vice versa,* the two being on the opposite sides of a metal tube.

It appears that four factors determine the rate of heat imparta-tion from the gas to any small area of the metal*:

(1).—The temperature difference between the body of the gas and the metal;

(2).—The weight of the gas per cubic foot, which is proportional to the number of molecules in any unit of volume;

(3).—The bodily velocity of the motion of the gas parallel to any small area under consideration; and (probably),

(4).—The specific heat of the gas at constant pressure.

The apparatus consists of an electric resistance furnace containing coils of nickel wire, a small (interchangeable) multi-tubular boiler, and a steam-jet apparatus for reducing the air pressure at the exit end, so as to cause a flow of air through the boiler. A surface condenser was attached to the boiler's steam outlet, the condensed steam being weighed as a check on the feed-water measurements. A number of thermometers and thermo-couples were used to obtain atmospheric-air temperature, temperatures of the air entering and leaving the boilers, and feed-water temperature.

The apparatus is now being reconstructed with appliances for measuring the quantity of air entering the furnace, and an automatic electric-furnace temperature regulator.

Three sizes of boiler have been tested thus far, the dimensions being as given in Table 4.

Each of the three boilers was tested at several temperatures of entering air, up to 1 500° Fahr., about ten tests being made at each temperature. It is also the intention to run, on these three boilers,

* The assumption is made that a metal tube free from scale will remain almost as cool as the water; actual measurements with thermo-couples have indicated the correctness of this assumption in the majority of cases.

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FIG. 1.—GAS PRODUCER, ECONOMIZER, AND WET SCRUBBER.

FIG. 2.—PRODUCER GAS: DRY SCRUBBER AND GAS HOLDER.

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about eight tests at temperatures of 1 800°, 2 100° and 2 400° Fahr., respectively. A bulletin on the work already done, together with much incidental matter, is in course of preparation.*

TABLE 4.—DIMENSIONS OF BOILERS NOS. 1, 2, AND 3.

Items.	Boiler No. 1.	Boiler No. 2.	Boiler No. 3.
Distance, outside to outside of boiler heads, in inches	8.28	8.28	16.125
Actual outside diameter of flues, in inches	0.252	0.313	0.252
Actual inside diameter of flues, in inches	0.175	0.230	0.175
Number of flues (tubes)	10	10	10

The work on the first three boilers is only a beginning; preparations are being made to test eight more multi-tubular boilers of various lengths and tube diameters, under similar conditions. Because of the experience already obtained, it will be necessary to make only eight tests at each initial air temperature.

When the work on multi-tubular boilers is completed, water-tube boilers will be taken up, for which a fairly complete outline has been prepared. This second or water-tube portion of the investigation is really of the greater scientific and commercial interest, but the multi-tubular boilers were investigated first because the mathematical treatment is much simpler.

Producer-Gas Tests.—The producer-gas plant at the Pittsburg testing station is in charge of Mr. Carl D. Smith, and has been installed for the purpose of testing low-grade fuel, bone coal, roof coal, mine refuse, and such material as is usually considered of little value, or even worthless for power purposes. The gas engine, gas producer, economizer, wet scrubber (Fig. 1, Plate XIX), and accessories, are in Building No. 13, and the dry scrubber, gas-holder, and water-cooling apparatus are immediately outside that building (Fig. 2, Plate XIX).

At present immense quantities of fuel are left at the mines, in the form of culm and slack, which, in quality, are much below the average output. Such fuel is considered of little or no value, chiefly because there is no apparatus in general use which can burn it to good advantage. The heat value of this fuel is often from 50 to 75% of that of the fuel marketed, and if not utilized, represents an im-

* "Heat Transmission into Steam Boilers," by W. T. Ray and H. Kreisinger, Bulletin (in press), U. S. Geological Survey.

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mense waste of natural resources. Large quantities of low-grade fuel are also left in the mines, simply because present conditions do not warrant its extraction, and it is left in such a way that it will be very difficult, if not practically impossible, for future generations to take out such fuel when it will be at a premium. Again, there are large deposits of low-grade coal in regions far remote from the sources of the present fuel supply, but where its successful and economic utilization would be a boon to the community and a material advantage to the country at large. The great importance of the successful utilization of low-grade fuel is obvious. Until within very recent years little had been accomplished along these lines, and there was little hope of ever being able to use these fuels successfully.

The development of the gas producer for the utilization of ordinary fuels,* however, indicates that the successful utilization of practically all low-grade fuel is well within the range of possibility. It is notable that, although all producer-gas tests at the Government testing stations, at St. Louis and Norfolk, were made in a type of producer† designed primarily for a good grade of anthracite coal, the fuels tested included a wide range of bituminous coals and lignites, and even peat and bone coal, and that, in nearly every test, little serious difficulty was encountered in maintaining satisfactory operating conditions.‡ It is interesting to note that in one test, a bone coal containing more than 45% of ash was easily handled in the producer, and that practically full load was maintained for the regulation test period of 50 hours.§

It is not expected that all the fuels tested will prove to be of immediate commercial value, but it is hoped that much light will be thrown on this important problem.

The equipment for this work consists of a single gas generator, rated at 150 h.p., and a three-cylinder, vertical gas engine of the same capacity. The producer is a Loomis-Pettibone, down-draft, made by the Power and Mining Machinery Company, of Cudahy, Wis., and is known as its "Type C" plant. The gas generator consists of a

* "The Producer Gas Power Plant," by R. H. Fernald, Bulletin No. 416, U. S. Geological Survey, 1909: also Professional Paper No. 48 and Bulletins Nos. 290, 316, 332, and 416.

† A Taylor up-draft pressure producer, made by R. D. Wood and Company, Philadelphia, Pa.

‡ "Coal Testing Plant, St. Louis, Mo.," by R. H. Fernald, Professional Paper No. 48, Vol. III. U. S. Geological Survey, 1906.

§ A report of these tests may be found in Bulletin No. * * *, U. S. Geological Survey.

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FIG. 1.—CHARGING FLOOR OF GAS PRODUCER.

FIG. 2.—EUROPEAN AND AMERICAN BRIQUETTES.

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cylindrical shell, 6 ft. in diameter, carefully lined with fire-brick, and having an internal diameter of approximately 4 ft. Near the bottom of the generator there is a fire-brick grate, on which the fuel bed rests. The fuel is charged at the top of the producer through a door (Fig. 1, Plate XX), which may be left open a considerable time without affecting the operation of the producer, thus enabling the operator to watch and control the fuel bed with little inconvenience. As the gas is generated, it passes downward through the hot fuel bed and through the fire-brick grate. This down-draft feature "fixes," or makes into permanent gases, the tarry vapors which are distilled from bituminous coal when it is first charged into the producer. A motor-driven exhauster with a capacity of 375 cu. ft. per min., draws the hot gas from the base of the producer through an economizer, where the sensible heat of the gas is used to pre-heat the air and to form the water vapor necessary for the operation of the producer. The pre-heated air and vapor leave the economizer and enter the producer through a passageway near the top and above the fuel bed. From the economizer the gas is drawn through a wet scrubber where it undergoes a further cooling and is cleansed of dirt and dust. After passing the wet scrubber, the gas, under a light pressure, is forced, by the exhauster, through a dry scrubber to a gas-holder with a capacity of about 1 000 cu. ft.

All the fuel used is carefully weighed on scales which are checked from time to time by standard weights; and, as the fuel is charged into the producer, a sample is taken for chemical analysis and for the determination of its calorific power. The water required for the generation of the vapor is supplied from a small tank carefully graduated to pounds; this observation is made and recorded every hour. All the water used in the wet scrubber is measured by passing it through a piston-type water meter, which is calibrated from time to time to insure a fair degree of accuracy in the measurement. Provision is made for observing the pressure and temperature of the gas at various points; these are observed and recorded every hour.

From the holder the gas passes through a large meter to the vertical three-cylinder Westinghouse engine, which is connected by a belt to a 175-kw., direct-current generator. The load on the generator is measured by carefully calibrated switch-board instruments, and is regulated by a specially constructed water rheostat which stands in front of the building.

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Careful notes are kept of the engine operation; the gas consumption and the load on the engine are observed and recorded every 20 min.; the quantity of jacket water used on the gas engine, and also its temperature entering and leaving the engine jackets, are recorded every hour. Indicator cards are taken every 2 hours. The work is continuous, and each day is divided into three shifts of 8 hours each; the length of a test, however, is determined very largely by the character and behavior of the fuel used.

A preliminary study of the relative efficiency of the coals found in different portions of the United States, as producers of illuminating gas, has been nearly completed under the direction of Mr. Alfred H. White, and a bulletin setting forth the results is in press.*

Tests of Liquid Fuels.—Tests of liquid fuels in internal-combustion engines, in charge of Mr. R. M. Strong, are conducted in the engine-room of Building No. 13.

The various liquid hydro-carbon fuels used in internal-combustion engines for producing power, range from the light refined oils, such as naphtha, to the crude petroleums, and have a correspondingly wide variation of physical and chemical properties.

The most satisfactory of the liquid fuels for use in internal-combustion engines, are alcohol and the light refined hydro-carbon oils, such as gasoline. These fuels, however, are the most expensive in commercial use, even when consumed with the highest practical efficiency, which, it is thought, has already been attained, as far as present types of engines are concerned.

At present little is known as to how far many of the very cheap distillates and crude petroleums can be used as fuel for internal-combustion engines. It is difficult to use them at all, regardless of efficiency.

Gasoline is comparatively constant in quality, and can be used with equal efficiency in any gasoline engine of the better grade. There are many makes of high-grade gasoline engines, tests on any of which may be taken as representative of the performance and action of gasoline in an internal-combustion engine, if the conditions under which the tests were made are clearly stated and are similar.

Kerosene varies widely in quality, and requires special devices for its use, but is a little cheaper than gasoline. It is possible that the

* "Illuminating Gas Coals," by A. H. White and Perry Barker, U. S. Geological Survey.

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kerosene engine may be developed so as to permit it to take the place of the smaller stationary and marine gasoline engines. This would mean considerable saving in fuel cost to the small power user, who now finds the liquid-fuel internal-combustion engine of commercial advantage. A number of engines at present on the market use kerosene; some use only the lighter grades and are at best comparatively less efficient than gasoline engines. All these engines have to be adjusted to the grade of oil to be used in order to get the best results.

Kerosene engines are of two general types: the external-vaporizer type, in which the fuel is vaporized and mixed with air before or as it is taken into the cylinder; and the internal-vaporizer type, in which the liquid fuel is forced into the cylinder and vaporized by contact with the hot gases or heated walls of a combustion chamber at the head of the cylinder. A number of special devices for vaporizing kerosene and the lighter distillates have been tried and used with some success. Heat is necessary to vaporize the kerosene as quickly as it is required, and the degree of heat must be held between the temperature of vaporization and that at which the oil will be carbonized. The vapor must also be thoroughly and uniformly mixed with air in order to obtain complete combustion. As yet, no reliable data on these limiting temperatures for kerosene and similar oils have been obtained. No investigation has ever been made of possible methods for preventing the oils from carbonizing at the higher temperatures, and the properties of explosive mixtures of oil vapors and air have not been studied. This field of engineering laboratory research is of vital importance to the solution of the kerosene-engine problem.

Distillates or fuel oils and the crude oils are much the cheapest of the liquid fuels, and if used efficiently in internal-combustion engines would be by far the cheapest fuels available in many large districts.

Several engine builders are developing kerosene vaporizers, which are built as a part of the engine, or are adapted to each different engine, as required to obtain the best results. Most of these vaporizers use the heat and the exhaust gases to vaporize the fuel, but they differ greatly in construction; some are of the retort type, and others are of the float-feed carburetter type. To what extent the lower-grade fuel oils can be used with these vaporizers is yet to be determined.

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There are only a few successful oil engines on the American market. The most prominent of these represent specific applications of the principal methods of internal vaporization, and all except one are of the hot-bulb ignition type. It will probably be found that no one of the 4-stroke cycle, or 2-stroke cycle, engines is best for all grades of oil, but rather that each is best for some one grade. The Diesel engine is in a class by itself, its cycle and method of control being somewhat different from the others.

An investigation of the comparative adaptability of gasoline and alcohol to use in internal-combustion engines, consisting of more than 2 000 tests, was made at the temporary fuel-testing plant of the Geological Survey, at Norfolk, Va., in 1907. A detailed report of these tests is in preparation.* A similar investigation of the comparative adaptability of kerosenes has been commenced, with a view to obtaining data on their economical use, leading up to the investigation of the comparative fuel values of the cheaper distillates and crude petroleum, as before discussed.

Washing and Coking Tests.—The investigations relating to the preparation of low-grade coals, such as those high in ash or sulphur, by processes that will give them a higher market value or increase their efficiency in use, are in charge of Mr. A. W. Belden. They include the washing and coking tests of coals, and the briquetting of slack and low-grade coal and culm-bank refuse so as to adapt these fuels for combustion in furnaces, etc.

This work has been conducted in the washery and coking plant temporarily located at Denver, Colo., and in Building No. 32 at the Pittsburg testing station, where briquetting is in progress. The details of these tests are set forth in the various bulletins issued by the Geological Survey.†

The washing tests are carried out in the following manner: As the raw coal is received at the plant, it is shoveled from the railroad cars to the hopper scale, and weighed. It then passes through the tooth-roll crusher, where the lumps are broken down to a maximum size of 2½ in. An apron conveyor delivers the coal to an elevator

* "Gasoline and Alcohol Tests," by R. M. Strong, Bulletin No. 392, U. S. Geological Survey, 1909.

† "Washing and Coking Tests," by Richard Moldenke, A. W. Belden and G. R. Delamater, Bulletin No. 336, U. S. Geological Survey, 1908; also, "Washing and Coking Tests at Denver, Colo.," by A. W. Belden and G. R. Delamater, Bulletin No. 368, U. S. Geological Survey, 1909.

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which raises it to one of the storage bins. As the coal is being elevated, an average sample representing the whole shipment is taken. An analysis is made of this sample of raw coal and float-and-sink tests are run to determine the size to which it is necessary to crush before washing, and the percentage of refuse with the best separation. From the data thus obtained, the washing machines are adjusted so that the washing test is made with full knowledge of the separations possible under varying percentages of refuse. The raw coal is drawn from the bin and delivered to a corrugated-roll disintegrator, where it is crushed to the size found most suitable, and is then delivered by the raw-coal elevator to another storage bin. The arrangement of the plant is such that the coal may be first washed on a Stewart jig, and the refuse then delivered to and re-washed on a special jig, or the refuse may be re-crushed and then re-washed.

When the coal is to be washed, it drops to the sluice box, where it is mixed with the water and sluiced to the jigs. In drawing off the washed coal, or when the uncrushed raw coal is to be drawn from a bin and crushed for the washing tests, however, a gate just below the coal-flow regulating gate is thrown in, and the coal falls into a central hopper instead of into the sluice box. Ordinarily, this gate forms one side of the vertical chute. The coal in this central hopper is carried by a chute to the apron conveyor, and thence to the roll disintegrator, or, in case it is washed coal, to a swing-hammer crusher. It will be noted that coal, in this manner, can be drawn from a bin at the same time that coal is being taken from another bin, and sluiced to the jigs for washing, the two operations not interfering in the least.

The washed coal, after being crushed and elevated to the top of the building, is conveyed by a chute to the coke-oven larry, and is weighed on the track scale, after which it is charged to the oven. The refuse is sampled and weighed as it is wheeled to the dump pile, and from this sample the analysis is made and a float-and-sink test run to determine the "loss of good coal" in the refuse and to show the efficiency of the washing test.

The coking tests have been conducted in a battery of two beehive ovens, one 7 ft. high and 12 ft. in diameter, the other, 6½ ft. high and 12 ft. in diameter. A standard larry with a capacity of 8 tons, and the necessary scales for weighing accurately the coal charged and

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coke produced, complete the equipment. The coal is usually run through a roll crusher which breaks it to about ½-in. size, or through a Pennsylvania hammer crusher. The fineness of the coals put through the hammer crusher varies somewhat, but the average, taken from a large number of samples, is as follows: Through ⅛-in. mesh, 100%; over 10-mesh, 31.43%; over 20-mesh, 24.29%; over 40-mesh, 22.86%; over 60-mesh, 10 per cent. The results of the coking tests are set forth in detail in the various publications issued on this subject.*

Tests of coke produced in the illuminating-gas investigations before referred to, and a study of commercial coking and by-product plants, are included in these investigations.

Briquetting Investigations.—These investigations are in charge of Mr. C. L. Wright, and are conducted in Building No. 32, which is of fire-proof construction, having a steel-skeleton frame work, reinforced-concrete floors, and 2-in. cement curtain walls, plastered on expanded-metal laths. In this building two briquetting machines are installed, one an English machine of the Johnson type, and the other a German lignite machine of very powerful construction.

The investigations include the possibility of making satisfactory commercial fuels from lignite or low-grade coals which do not stand shipment well, the benefiting of culm or slack coals which are wasted or sold at unremunerative prices, and the possibility of improving the efficiency of good coals. Some of the various forms of commercial briquettes, American and foreign, are shown in Fig. 2, Plate XX. After undergoing chemical analysis, the coal is elevated and fed to a storage bin, whence it is drawn through a chute to a hopper on the weighing scales. There it is mixed with varying percentages of different kinds of binding material, and the tests are conducted so as to ascertain the most suitable binder for each kind of fuel, which will produce the most durable and weather-proof briquette at least cost, and the minimum quantity necessary to produce a good, firm briquette. After weighing, the materials to be tested are run through the necessary grinding and pulverizing machines and are fed into the briquetting machines, whence the manufactured briquettes are delivered for loading or storage. The materials to be used in the German machine are also dried and cooled again.

* U. S. Geological Survey, Professional Paper No. 48, Pt. III, and Bulletins Nos. 290, 332, 336, 368, 385, and 403.

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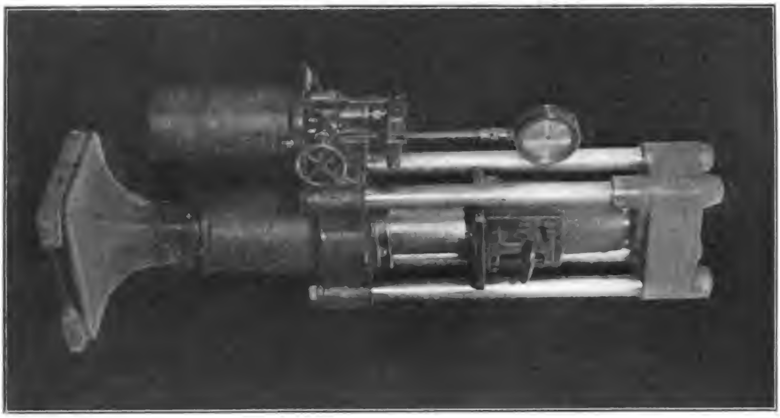

FIG. 1.—HAND BRIQUETTING PRESS.

FIG. 2.—COAL BRIQUETTING MACHINE.

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The briquettes made at this plant are then subjected to physical tests in order to determine their weathering qualities and their resistance to abrasion; extraction tests and chemical analyses are also made. Meanwhile other briquettes from the same lots are subjected to combustion tests for comparison with the same coal not briquetted. These tests are made in stationary boilers, in house-heating boilers, on locomotives, naval vessels, etc., and the results, both of the processes of manufacture, and of the tests, are published in various bulletins issued by the Geological Survey.*

The equipment includes storage bins for the raw coal, scales for weighing, machines for crushing or cracking the pitch, grinders, crushers, and disintegrators for reducing the coal to the desired fineness, heating and mixing apparatus, presses and moulds for forming the briquettes, a Schulz drier, and a cooling apparatus.

There is a small experimental hand-briquetting press (Fig. 1, Plate XXI) for making preliminary tests of the briquetting qualities of the various coals and lignites. With this it is easily possible to vary the pressure, heat, percentage and kind of binder, so as to determine the best briquetting conditions for each fuel before subjecting it to large-scale commercial tests in the big briquetting machines.

This hand press will exert pressures up to 50 tons or 100 000 lb. per sq. in., on a plunger 3 in. in diameter. This plunger enters a mould, which can be heated by a steam jacket supplied with ordinary saturated steam at a pressure of 125 lb., and compresses the fuel into a briquette, 8 in. long, under the conditions of temperature and pressure desired.

The Johnson briquetting machine, which requires 25 h.p. for its operation, exerts a pressure of about 2 500 lb. per sq. in., and makes briquettes of rectangular form, 6¾ by 4¼ by 2½ in., and having an average weight of about 3¾ lb. The capacity of the machine (Fig. 2, Plate XXI) is about 3.8 tons of briquettes per 8-hour day.

Under the hopper on the scales for the raw material is a square wooden reciprocal plunger which pushes the fuel into a hole in the floor at a uniform rate. The pitch is added as uniformly as possible by hand, as the coal passes this hole. Under this hole a horizontal screw conveyor carries the fuel and pitch to the disintegrator, in front

* "Professional Paper No. 48, and Bulletins Nos. 290, 316, 332, 343, 363, 366, 385, 402, 403, and 412," U. S. Geological Survey.

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of which, in the feeding chute, there is a powerful magnet for picking out any pieces of iron which might enter the machine and cause trouble.

The ground mixture is elevated from the disintegrator to a point above the top of the upper mixer of the machine. At the base of this cylinder, steam can be admitted by several openings to heat the material to any desired temperature, usually from 180° to 205° Fahr. There, a plunger, making 17 strokes per min., compresses two briquettes at each stroke.

The German lignite-briquetting machine (Figs. 18 and 19) was made by the Maschinenfabrik Buckau Actien-Gesellschaft, Magdeburg, Germany. Lignite from the storage room on the third floor of the building is fed into one end of a Schulz tubular drier (Fig. 1, Plate XXII), which is similar to a multi-tubular boiler set at a slight angle from the horizontal, and slowly revolved by worm and wheel gearing, the lignite passing through the tubes and the steam being within the boiler. From this drier the lignite passes through a sorting sieve and crushing rolls to a cooling apparatus, which consists of four horizontal circular plates, about 13 ft. in diameter, over which the dried material is moved by rakes. After cooling, the material is carried by a long, worm conveyor to a large hopper over the briquette press, and by a feeding box to the press (Fig. 2, Plate XXII).

The press, which is of the open-mould type, consists of a ram and die plates, the latter being set so as to make a tube which gradually tapers toward the delivery end of the machine. The briquettes have a cross-section similar to an ellipse with the ends slightly cut off; they are about 1¼ in. thick and average about 1 lb. in weight (Fig. 2, Plate XX). The press is operated by a direct connection with a steam engine of 150 h.p., the base of which is continuous with that of the press. The exhaust steam from the engine is used to heat the driver.

The plunger makes from 80 to 100 strokes per min., the pressure exerted ranging from 14 000 to 28 000 lb. per sq. in., the capacity of the machine being 1 briquette per stroke, or from 2½ to 3 tons of completed briquettes per hour. It is expected that no binder will be needed for practically all the brown lignite briquetted by this machine, thus reducing the cost as compared with the briquetting of coals, which require from 5 to 7% of water-gas, pitch binder costing more than 50 cents per ton of manufactured briquettes.

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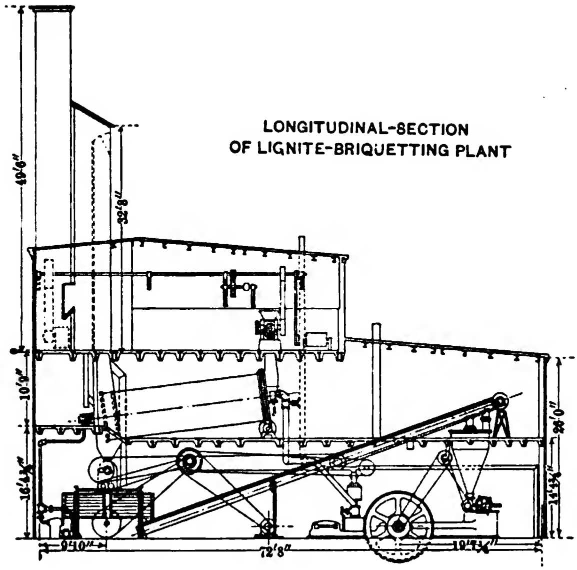

FIG. 18.

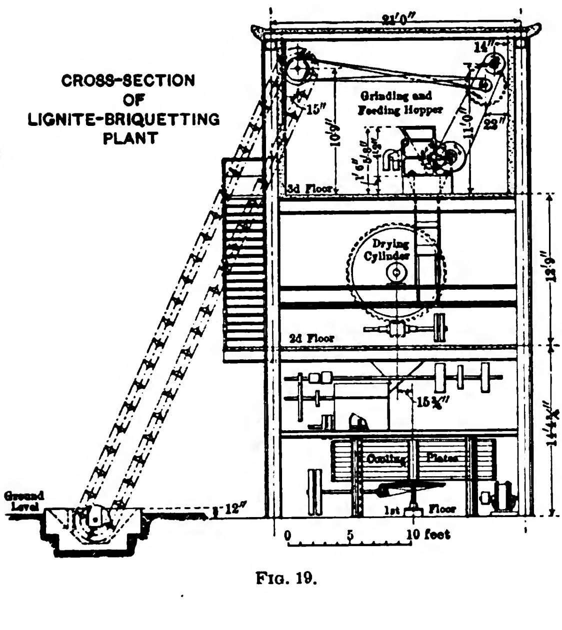

FIG. 19.

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Peat Investigations.—Investigations into the distribution, production, origin, nature, and uses of peat are being conducted by Mr. C. A. Davis, and include co-operative arrangements with State Geological Surveys and the Geologic Branch of the U. S. Geological Survey. These organizations conduct surveys which include the mapping of the peat deposits in the field, the determination of their extent and limitations, the sampling of peat from various depths, and the transmittal of samples to the Pittsburg laboratories for analysis and test.*

This work is co-ordinated in such a manner as to result in uniform methods of procedure in studying the peat deposits of the United States. The samples of peat are subjected to microscopic examination, in order to determine their origin and age, and to chemical and physical tests at the laboratories in Pittsburg, so as to ascertain the chemical composition and calorific value, the resistance to compressive strains, the ash and moisture content, drying properties, resistance to abrasion, etc. Occasionally, large quantities of peat are disintegrated and machined, and portions, after drying for different periods, are subjected to combustion tests in steam boilers and to tests in the gas producer, to ascertain their efficiency as power producers.

Results.—The full value of such investigations as have been described in the preceding pages cannot be realized for many years; but, even within the four years during which this work has been under way, certain investigations have led to important results, some of which may be briefly mentioned:

The chemical and calorific determinations of coals purchased for the use of the Government have resulted in the delivery of a better grade of fuel without corresponding increase in cost, and, consequently, in saving to the Government. Under this system, of purchasing its coal under specifications and testing, the Government is getting more nearly what it pays for and is paying for what it gets. These investigations, by suggesting changes in equipment and methods, are also indicating the practicability of the purchase of cheaper fuels, such as bituminous coal and the smaller sizes of pea, buckwheat, etc., instead of the more expensive sizes of anthracite, with a corresponding saving in cost. The Government's fuel bill now aggregates about $10 000 000 yearly.

* "Peat Deposits of Maine," by E. D. Bastin and C. A. Davis. Bulletin No. 376, U. S. Geological Survey, 1909.

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FIG. 1.—DRYER FOR LIGNITE BRIQUETTING PRESS.

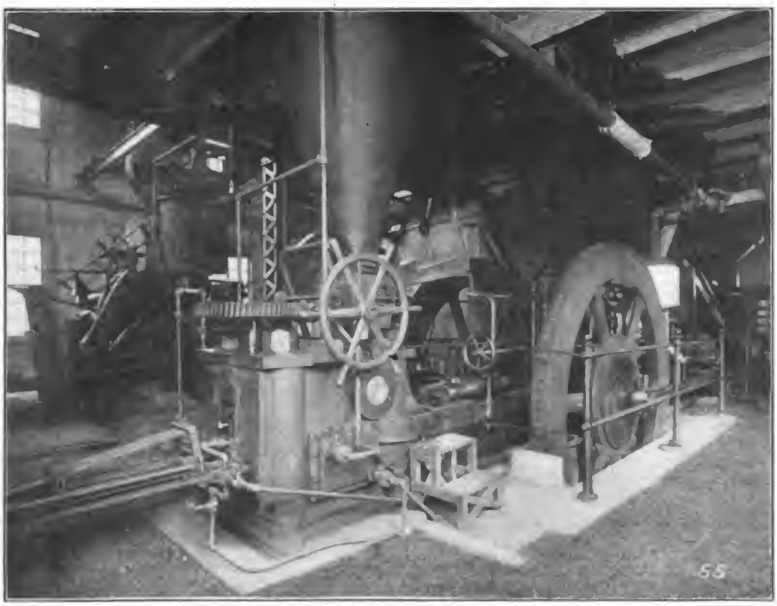

FIG. 2.—LIGNITE BRIQUETTING MACHINE.

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The making and assembling of chemical analyses and calorific determinations (checked by other tests) of carefully selected samples of coals from nearly 1 000 different localities, in the different coal fields of the United States, with the additions, from time to time, of samples representing parts of coal fields or newly opened beds of coal in the same field, furnish invaluable sources of accurate information, not only for use of the Government, but also for the general public. Of the above-mentioned localities, 501 were in the public-land States and 427 in the Central, Eastern, and Southern States.

The chemical anaylses of the coals found throughout the United States have been made with such uniformity of method, both as to collection of samples and analytical procedure, as to yield results strictly comparable for coals from all parts of the country, and furnish complete information, as a basis for future purchases and use by the Government and by the general public, of all types of American coals.

Other researches have resulted in the acquirement of valuable information regarding the distribution of temperature in the fuel bed of gas producers and furnaces, showing a range of from 400° to 1 300° cent., and have thus furnished data indicating specific difficulties to be overcome in gas-producer improvements for greater fuel efficiency.

The recent studies of the volatile matter in coal, and its relation to the operation of coke ovens and other forms of combustion, have demonstrated that as much as one-third of this matter is inert and non-combustible, a fact which may have a direct bearing on smoke prevention by explaining its cause and indicating means for its abatement.

Experiments in the storage of coal have proven that oxygen is absorbed during exposure to air, thereby causing, in some cases, a deterioration in heating value, and indicating that, for certain coals, in case they are to be stored a long time for naval and other purposes, storage under water is advisable.

The tests of different coals under steam boilers have shown the possibility of increasing the general efficiency of hand-fired steam boilers from 10 to 15% over ordinary results. If this saving could be made in the great number of hand-fired boilers now being operated in all parts of the United States, it would result in large saving in the fuel bill of the country. Experiments which have been made with residence-heating boilers justify the belief that it will be possible

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to perfect such types of boilers as may economically give a smokeless operation. The tests under steam boilers furnish specific information as to the most efficient method of utilizing each of a number of different types of coal in Government buildings and power plants in different parts of the country.

The tests in the gas producer have shown that many fuels of such low grade as to be practically valueless for steam-furnace purposes, including slack coal, bone coal, and lignite, may be economically converted into producer gas, and may thus generate sufficient power to render them of high commercial value.

Practically every shipment out of several hundred tested in the gas producers, including coals as high in ash content as 45%, and lignites and peats high in moisture, has been successfully converted into producer gas which has been used in operating gas engines. It has been estimated that on an average there was developed from each coal tested in the gas-producer plant two and one-half times the power developed when used in the ordinary steam-boiler plant, and that such relative efficiencies will probably hold good for the average plant of moderate power capacity, though this ratio may be greatly reduced in large steam plants of the most modern type. It was found that the low-grade lignites of North Dakota developed as much power, when converted into producer gas, as did the best West Virginia bituminous coals when utilized under the steam boiler; and, in this way, lignite beds underlying from 20 000 000 to 30 000 000 acres of public lands, supposed to have little or no commercial value, are shown to have a large value for power development.

The tests made with reference to the manufacture and combustion of briquetted coal have demonstrated conclusively that by this means many low-grade bituminous coals and lignites may have their commercial value increased to an extent which more than covers the increased cost of making; and these tests have also shown that bituminous coals of the higher grades may be burned in locomotives with greatly increased efficiency and capacity and with less smoke than the same coal not briquetted. These tests have shown that, with the same fuel consumption of briquettes as of raw coal, the same locomotive can very materially increase its hauling capacity and thus reduce the cost of transportation.

The investigations into smoke abatement have indicated clearly

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that each type of coal may be burned practically without smoke in some type of furnace or with some arrangement of mechanical stoker, draft, etc. The elimination of smoke means more complete combustion of the fuel, and consequently less waste and higher efficiency.

The investigations into the waste of coal in mining have shown the enormous extent of this waste, aggregating probably from 300 000 000 to 400 000 000 tons yearly, of which at least one-half might be saved. It is being demonstrated that the low-grade coals, high in sulphur and ash, now left underground, can be used economically in the gas producer for power and light, and, therefore, should be mined at the same time that the high-grade coal is being removed. Moreover, attention is now being called to the practicability of a further large reduction of waste through more efficient mining methods.

The washing tests have demonstrated the fact that many coals, too high in ash and sulphur for economic use under the steam boiler or for coking, may be rendered of commercial value by proper treatment in the washery. The coking tests have also demonstrated that, by proper methods of preparation for and manipulation in the beehive oven, many coals which were not supposed to be of economic value for coking purposes, may be rendered so by prior washing and proper treatment. Of more than 100 coals tested during 1906 from the Mississippi Valley and the Eastern States, most of which coals were regarded as non-coking, all except 6 were found, by careful manipulation, to make fairly good coke for foundry and other metallurgical purposes. Of 52 coals from the Rocky Mountain region, all but 3 produced good coke under proper treatment, though a number of these had been considered non-coking coals.

Investigations into the relative efficiency of gasoline and denatured alcohol as power producers, undertaken in connection with work for the Navy Department, have demonstrated that with proper manipulation of the carburetters, igniters, degree of compression, etc., denatured alcohol has the same power-producing value, gallon for gallon, as gasoline. This is a most interesting development, in view of the fact that the heat value of a gallon of alcohol is only a little more than 0.6 that of a gallon of gasoline. To secure these results, compressions of from 150 to 180 lb. per sq. in. were used, these pressures involving an increase in weight of engine. Although the engine especially de-

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signed for alcohol will be heavier than a gasoline engine of the same size, it will have a sufficiently greater power capacity so that the weight per horse-power need not be greater.

Several hundred tons of peat have been tested to determine methods of drying, compressing into briquettes, and utilization for power production in the gas producer. In connection with these peat investigations, a reconnoissance survey has been made of the peat deposits of the Atlantic Coast. Samples have been obtained by boring to different depths in many widely distributed peat-bogs, and these samples have been analyzed and tested in order to determine their origin, nature, and fuel value.

The extent and number of tests from which these results have been derived will be appreciated from the fact that, in three years, nearly 15 000 tests were made, in each of which large quantities of fuel were consumed. These tests involved nearly 1 250 000 physical observations and 67 080 chemical determinations, made with a view to analyze the results of the tests and to indicate any necessary changes in the methods as they progressed. For coking, cupola, and washing, 596 tests, of which nearly 300 involved the use of nearly 1 000 tons of coal, have been made at Denver. For briquetting, 312 tests have been made. Briquettes have been used in combustion tests in which 250 tons of briquetted coal were consumed in battleship tests, 210 tons in torpedo-boat tests, 320 tons in locomotive tests on three railway systems, and 70 tons were consumed under stationary steam boilers. Of producer gas tests, 175 have been made, of which 7 were long-time runs of a week or more in duration, consuming in all 105 tons of coal. There have been 300 house-heating boiler tests and 575 steam-boiler tests; also, 83 railway-locomotive and 23 naval-vessel tests have been made on run-of-mine coal in comparison with briquetted coal; also, 125 tests have been made in connection with heat-transmission experiments, and 2 254 gasoline- and alcohol-engine tests. Nearly 10 000 samples of coal were taken for analysis, of which 3 000 were from public-land States. Nearly 5 000 inspection samples, of coal purchased by the Government for its use, have been taken and tested.

The results of the tests made in the course of these investigations, as summarized, have been published in twelve separate Bulletins, three of which, Nos. 261, 290, and 332, set forth in detail the operations of the fuel-testing plant for 1904, 1905, and 1906. Professional

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Paper No. 48, in three volumes, describes in greater detail each stage of the operations for 1904 and 1905.

Separate Bulletins, descriptive of the methods and results of the work in detail, have been published, as follows: No. 323, Experimental work conducted in the chemical laboratory; No. 325, A study of four hundred steaming tests; No. 334, Burning of coal without smoke in boiler plants; No. 336, Washing and coking tests of coal, and cupola tests of coke; No. 339, Purchase of coal under specifications on basis of heating value; No. 343, Binders for coal briquettes; No. 362, Mine sampling and chemical analyses of coals in 1907; No. 363, Comparative tests of run-of-mine and briquetted coal on locomotives, including torpedo-boat tests, and some foreign specifications for briquetted fuel; No. 366, Tests of coal and briquettes as fuel for house-heating boilers; No. 367, Significance of drafts in steam-boiler practice; No. 368, Coking and washing tests of coal at Denver; No. 373, Smokeless combustion of coal in boiler plants, with a chapter on central heating plants; No. 378, Results of purchasing coal under Government specifications; No. 382, The effect of oxygen in coal; and, No. 385, Briquetting tests at Norfolk, Va.

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DISCUSSION

Mr. Allen. KENNETH ALLEN, M. AM. SOC. C. E.—The speaker would like to know whether anything has been done in the United States toward utilizing marsh mud for fuel.

In an address by Mr. Edward Atkinson, before the New England Water Works Association, in 1904, on the subject of "Bog Fuel," he referred to its extensive use in Sweden and elsewhere, and intimated that there was a wide field for its use in America.

The percentage of combustible material in the mud of ordinary marsh lands is very considerable, and there are enormous deposits readily available; but it is hardly probable that its calorific value is sufficiently high to render its general use at this time profitable.

As an example of the amount of organic matter which may remain stored in these muds for many years, the speaker would mention a sample taken from the bottom of a trench, which he had analyzed a few years ago. Although taken from a depth of about 15 ft., much of the vegetable fiber remained intact. The material proved to be $70\frac{3}{4}\%$ volatile.

Possibly before the existing available coal deposits are exhausted, the exploitation of meadow muds for fuel may become profitable.

Mr. Kreisinger. HENRY KREISINGER, ESQ.* (by letter).—Mr. Wilson gives a brief description of a long furnace and an outline of the research work which is being done in it. It may be well to discuss somewhat more fully the proposed investigations and point out the practical value of the findings to which they may lead.

In general, the object is to study the process of combustion of coal. When soft coal is burned in any furnace, part of the combustible is driven off shortly after charging, and has to be burned in the space between the fuel bed and the exit of the gases, which is called the combustion space. There is enough evidence to show that, with a constant air supply, the completeness of the combustion of the volatile combustible depends on the length of time the latter stays within the combustion space; but, with a constant rate of charging the coal, this length of time depends directly on the extent of the combustion space. Thus, if the volume of the volatile combustible evolved per second and the admixed air is 40 cu. ft., and the extent of the combustion space is 80 cu. ft., the average time the gas will stay within the latter is 2 sec.; if the combustion space is 20 cu. ft., the average time the mixture can stay in this space is only $\frac{1}{2}$ sec., and its combustion will be less complete than in the first case. Thus it is seen that the extent of the combustion space of a furnace is an important factor in the economic combustion of volatile coals. The specific object of the

* U. S. Geological Survey, Pittsburg, Pa.

Mr. Kreisinger.

investigations, thus far planned, is to determine the extent of the combustion space required to attain practically complete combustion when a given quantity of a given coal is burned under definite conditions. With this object in view, the furnace has been provided with a combustion space large enough for the highest volatile coals and for the highest customary rate of combustion. To illustrate the application of the data which will be obtained by these experiments, the following queries are given:

Suppose it is required to design a furnace which will burn coal from a certain Illinois mine at the rate of 1 000 lb. per hour, with a resulting temperature of not less than 2 800° Fahr. How large a combustion space is required to burn, with practical completeness, the volatile combustible? What completeness of combustion can be attained, if the combustion space is only three-fourths of the required extent? In the present state of the knowledge of the process of combustion of coal, these queries cannot be answered definitely. In the literature on combustion one may find statements that the gases must be completely burned before leaving the furnace or before they strike the cooling surfaces of the boiler; but there is no definite information available as to how long the gases must be kept in the furnace or how large the combustion space must be in order to obtain practically complete combustion. It is strange that so little is known of such an old art as the combustion of coal.

The research work under consideration is fundamentally a problem in physical chemistry, and, for that reason, has been assigned to a committee consisting of the writer as Engineer, Dr. J. C. W. Frazer, Chemist, and Dr. J. K. Clement, Physicist. The outcome of the investigation may prove of extreme interest to mechanical and fuel engineers, and to all who have anything to do with the burning of coal or the construction of furnaces. In the experiments thus far planned the following factors will be considered:

Effect of the Nature of Coal on the Extent of Combustion Space Required.—The steaming coals mined in different localities evolve different volumes of volatile combustible, even when burned at the same rate. The coal which analyzes 45% of volatile matter evolves a much greater volume of gases and tar vapors than that analyzing only 15 per cent. These evolved gases and tar vapors must be burned in the space. Consequently, a furnace burning high volatile coal must have a much larger combustion space than that burning coal low in volatile combustible.

There is enough evidence to show that the extent of combustion space required to burn the volatile combustible depends, not only on the volume of the combustible mixture, but also on the chemical composition of the volatile combustible. Thus the volatile combustible of

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Mr. Kreisinger.

low volatile coal, when mixed with an equal volume of air, may require 1 sec. in the combustion space to burn practically to completeness, while it may require 2 sec. to burn the same volume of the volatile combustible of high volatile coal with the same completeness; so that the extent of the combustion space required to burn various kinds of coal may not be directly proportional to the volatile matter of the coal.

Effect of the Rate of Combustion on the Extent of Combustion Space Required.—With the same coal, the volume of the volatile combustible distilled from the fuel bed per unit of time varies as the rate of combustion. Thus, when this rate is double that of the standard, the volume of gases and tar vapors driven from the fuel is about doubled. To this increased volume of volatile combustible, about double the volume of air must be added, and, if the mixture is to be kept the same length of time within the combustion space, the latter should be about twice as large as for the standard rate of combustion. Thus the combustion space required for complete combustion varies, not only with the nature of the coal, but also with the rate of firing the fuel, which, of course, is self-evident.

Effect of Air Supply on the Extent of Combustion Space Required.—Another factor which influences the extent of the combustion space is the quantity of air mixed with the volatile combustible. Perhaps, within certain limits, the combustion space may be decreased when the supply of air is increased. However, any statement at present is only speculation; the facts must be determined experimentally. One fact is known, namely, that, in order to obtain higher temperatures of the products of combustion, the air supply must be decreased.

Effect of Rate of Heating of Coal on the Extent of Combustion Space Required.—There is still another factor, a very important one, which, with a given coal and any given air supply, will influence the extent of the combustion space. This factor is the rate of heating of the coal when feeding it into the furnace. The so-called "proximate" analysis of coal is indeed only very approximate. When the analysis shows, say, 40% of volatile matter and 45% of fixed carbon, it does not mean that the coal is actually composed of so much volatile matter and so much fixed carbon; it simply means that, under a certain rate of heating attained by certain standard laboratory conditions, 40% of the coal has been driven off as "volatile matter." If the rate or method of heating were different, the amount of volatile matter driven off would also be different. Chemists state that it is difficult to obtain accurate checks on "proximate" analysis. To illustrate this factor, further reference may be made to the operation of the up-draft bituminous gas producers. In the generator of such producers the tar vapors leave the freshly fired fuel, pass through the wet scrubber, and are finally separated by the tar extractor as a black, pasty substance in a semi-liquid state. If this tar is subjected to the standard proximate

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analysis, it will be shown that from 40 to 50% of it is fixed carbon, although it left the gas generator as volatile matter. It is desired to emphasize the fact that different rates of heating of high volatile coals will not only drive off different percentages of volatile matter, but that the latter itself varies greatly in chemical composition and physical properties as regards inflammability and rapidity of combustion. Thus it may be said that the extent of the combustion space required for the complete oxidation of the volatile combustible depends on the method of charging the fuel, that is, on how rapidly the fresh fuel is heated. If this factor is given proper consideration, it may be possible to reduce very materially the necessary space required for complete combustion. Mr. Kreisinger.

The Effect of the Rate of Mixing the Volatile Combustible and Air on the Extent of the Combustion Space.—When studying the effects discussed in the preceding paragraphs, the rate of mixing the volatile combustible with the supply of air must be as constant as practicable. At first, tests will be made with no special mixing devices, the mixing will be accomplished entirely by the streams of air entering the furnace at the stoker, and by natural diffusion. Although there appears to be violent stirring of the gases above the fuel bed, the mixture of the gases does not become homogeneous until they are about 10 or 15 ft. from the stoker. The mixing caused by the air currents forced into the furnace at the stoker is very distinct, and can be readily observed through the peep-hole in the side wall of the Heine boiler, opposite the long combustion chamber. This mixing is shown in Fig. 20. *A* is a current of air forced from the ash-pit directly upward through the fuel bed; *B* and *B* are streams of air forced above the fuel bed through numerous small openings at the furnace side of each hopper. Those currents cause the gases to flow out of the furnace in two spirals, as shown in Fig. 20. The velocity of rotation on the outside of the two spirals appears to be about 10 ft. per sec., when the rate of combustion is about 750 lb. of coal per hour. It is reasonable to expect that when the rate of mixing is increased by building piers and other mixing structures immediately back of the grate, the completeness of the combustion will be effected in less time, and a smaller combustion space will be required. Thus, the mixing structures may be an important factor in the extent of the required combustion space.

To sum up, it can be said that the extent of the space required to obtain a combustion which can be considered complete for all practical purposes, depends on the following factors:

(*a*).—Nature of coal,
(*b*).—Rate of combustion,
(*c*).—Supply of air,
(*d*).—Rate of heating fuel,
(*e*).—Rate of mixing volatile combustible and air.

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Mr. Kreisinger.

Just how much the extent of the combustion space required will be influenced by these factors is the object of the experiments under discussion.

The Scope of the Experiments.—With this object in view, as explained in the preceding paragraphs, the following series of experiments are planned:

Six or eight typical coals are to be selected, each representing a certain group of nearly the same chemical composition. Each series will consist of several sets of tests, each set being run with all the condi-

SECTION THROUGH STOKER SHOWING MIXING OF GASES CAUSED BY CURRENTS OF AIR

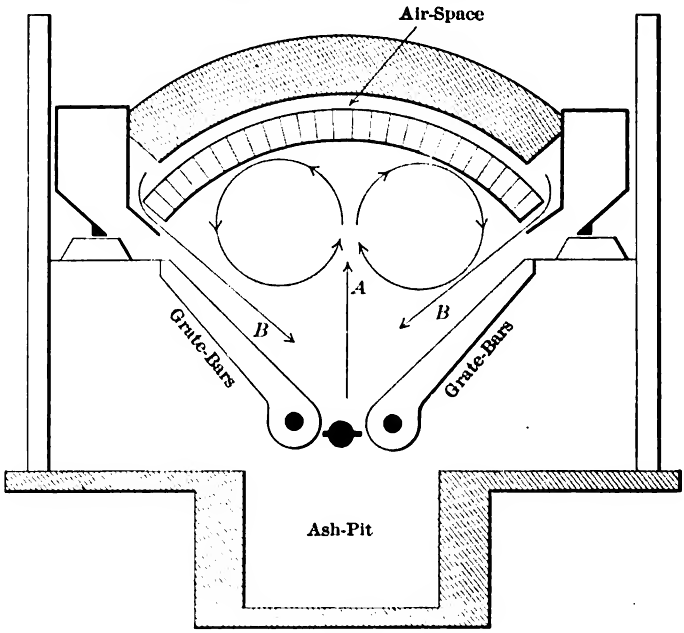

FIG. 20.

tions constant except the one, the effect of which on the size of the combustion space is to be investigated. Thus a set of four or five tests will be made, varying in rate of combustion from 20 to 80 lb. of coal per square foot of grate per hour, keeping the supply of air per pound of combustible and the rate of heating constant. This set will show the effect of the rate of combustion of the coal on the extent of space required to obtain combustion which is practically complete. Other variables, such as composition of coal, supply of air, and rate of heating, remain constant.

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Another set of four or five tests will be made with the same coal and at the same rate of combustion, but the air supply will be different for each test. This set of tests will be repeated for two or three different rates of combustion. Thus each of these sets will give the effect of the air supply on the extent of combustion space when the coal and rate of combustion remain constant. Mr. Kreisinger.

Still another set of tests should be made in which the time of heating the coal when feeding it into the furnace will vary from 3 to 30 min. In each of the tests of this set, the rate of combustion and the air supply will be kept constant, and the set will be repeated for two or three rates of combustion and two or three supplies of air. Each of these sets of tests will give the effect of the rate of heating of fresh fuel on the extent of combustion space required to burn the distilled volatile combustible. These sets of experiments will require a modification in the stoker mechanism, and, on that account, may be put off until all the other tests on the other selected typical coals are completed. As the investigation proceeds, enough may be learned so that the number of tests in each series may be gradually reduced. After all the desirable tests are made with the furnace as it stands, several kinds of mixing structures will be built successively back of the stoker and tried, one kind at a time, with a set of representative tests. Thus the effectiveness of such mixing structures will be determined.

Determining the Completeness of Combustion.—The completeness of combustion in the successive cross-sections of the stream of gases is determined mainly by the chemical analysis of samples of gases collected through the openings at these respective cross-sections. The first of these cross-sections at which gas samples are collected, passes through the middle of the bridge wall; the others are placed at intervals of 5 ft. through the entire length of the furnace. Measurements of the temperature of the gases, and direct observations of the length and color of the flames and of any visible smoke will be also made through the side peep-holes. These direct observations, together with the gas analysis, will furnish enough data to determine the length of travel of the combustible mixture to reach practically complete combustion.

In other words, these observations will determine the extent of the combustion space for various kinds of coal when burned under certain given conditions. Direct observations and the analysis of gases at sections nearer the stoker than that at which the combustion is practically complete, will show how the process of combustion approaches its completion. This information will be of extreme value in determining the effect of shortening the combustion space on the loss of heat due to incomplete combustion.

Method of Collecting Gas Samples.—The collection of gas samples is a difficult problem in itself, when one considers that the temperature

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Mr. Kreisinger. of the gases, as they are in the furnace, ranges from 2 400° to 3 200° Fahr.; consequently, the samples must be collected with water-cooled tubes. Thus far, about 25 preliminary tests have been made. These tests show that the composition of the gases at the cross-sections near the stoker is not uniform, and that more than one sample must be taken from each cross-section. It was decided to take 9 samples from the cross-section immediately back of the stoker, and reduce the number in the sections following, according to the uniformity of the gas composition. Thus, about 35 simultaneous gas samples must be taken for each test. The samples will be subjected, not only to the usual determination of CO_2, O_2 and CO, but to a complete analysis. It is also realized that some of the carbon-hydrogen compounds which, at the furnace temperature, exist as heavy gases, are condensed to liquids and solids when cooled in the sampling tubes, where they settle and tend to clog it. To neglect the presence of this form of the combustible would introduce considerable error in the determination of the completeness of combustion at any of the cross-sections. Therefore, special water-cooled sampling tubes are constructed and equipped with filters which separate the liquid and solid combustible from the gases. The contents of these filters are then also subjected to complete analysis. To obtain quantitative data, a measured quantity of gases must be drawn through these filtering sampling tubes.

The Measuring of Temperatures.—At present the only possible known method of measuring the temperature of the furnace gases is by optical and radiation pyrometers. Platinum thermo-couples are soon destroyed by the corrosive action of the hot gases. The pyrometers used at present are the Wanner optical pyrometer and the Fery radiation pyrometer.

The Flow of Heat Through Furnace Walls.—An interesting side investigation has developed, in the study of the loss of heat through the furnace walls. In the description of this experimental furnace it has been said that the side walls contained a 2-in. air space, which, in the roof, was replaced with a 1-in. layer of asbestos. To determine the relative resistance to heat flow of the air space and the asbestos layer, 20 thermo-couples were embedded, in groups of four, to different depths at three places in the side wall and at two places in the roof. In the side wall, one of the thermo-couples of each group was placed in the inner wall near the furnace surface; the second thermo-couple was placed in the same wall, but near the surface facing the air space; the third thermo-couple was placed in the outer wall near the inner surface; and the fourth was placed near the outer surface in the outer wall. In the roof the second and third thermo-couples were placed in the brick near the surface on each side of the asbestos layer. These thermo-couples have shown that the temperature drop across the 2-in. air space was much less than that across the 1-in. layer of asbestos;

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in fact, that it was considerably less than the temperature drop through the same thickness of the brick wall. Mr. Kreisinger.

The results obtained prove that, as far as heat insulation is concerned, air spaces in furnace walls are undesirable. The heat is not conducted through the air, but leaps across the space by radiation. In furnace construction a solid wall is a better heat insulator than one of the same total thickness containing an air space. If it is necessary to build a furnace wall in two parts on account of unequal expansion, the space between the two walls should be filled with some solid, cheap, non-conducting materials, such as ash, sand, or crushed brick. A more detailed account of these experiments may be found in a Bulletin of the U. S. Geological Survey entitled "The Flow of Heat Through Furnace Walls."

Walter O. Snelling, Esq.* (by letter).—The work of the United States Testing Station at Pittsburg has been set forth so fully by Mr. Wilson that a further statement as to the results achieved may seem like repetition. It would be most unlikely, however, that studies of such variety should possess no other value than along the direct lines being investigated. In the case of the Mine Accidents Division, at least, it is certain that the indirect benefits of some of the studies have been far-reaching, and are now proving of value in lines far removed from those which were the primary object of the investigation. They are developing facts which will be of great value to all engineers or contractors engaged in tunneling or quarrying. As the writer's experience has been solely in connection with the chemical examination of explosives, he will confine his discussion to this phase. Mr. Snelling.

In studying the properties of various explosives, and in testing work to separate those in which the danger of igniting explosive mixtures of coal dust and air, or of fire-damp and air, is greatest, from those in which this danger is least, much information has been collected. Mr. Wilson has described many of the tests, and it can be readily seen that in carrying out these and other tests on each of the explosives submitted, a great many facts relating to the properties of explosive compounds have been obtained, which were soon found to be of decided value in directions other than the simple differentiation of explosives which are safe from those which are unsafe in the presence of explosive mixtures of fire-damp or coal dust.

The factors which determine the suitability of an explosive for work in material of any particular physical characteristics depend on the relationship of such properties as percussive force (or the initial blow produced by the products of the decomposition of the explosive at the moment of explosion), and the heaving force (or the continued pressure produced by the products of the decomposition, after the

* Chief Explosives Chemist, U. S. Geological Survey.

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Mr. Snelling.

initial blow at the instant of detonation). Where an explosive has been used in coal or rock of a certain degree of brittleness, and where the work of the explosive with that particular coal is not thoroughly satisfactory, it becomes evident that through the systematic use of the information available at the Testing Station (and now in course of publication in the form of bulletins), in regard to the relationship between percussive and heaving forces in different explosives, as shown by the tests with small lead blocks, the Trauzl test, and the ballistic pendulum, that explosives can be selected which, possessing in modified form the properties of the explosive not entirely satisfactory in that type of coal or rock, would combine all the favorable properties of the first explosive, together with such additional advantages as would come from its added adaptation to the material in which it is to be used.

For example, if the explosive in use were found to have too great a shattering effect on the coal, an examination of the small lead-block test of this explosive, and a comparison of this with lead-block tests of other explosives having practically the same strength, as shown by the ballistic pendulum, will enable the mine manager to select from those already on the Permissible List (and therefore vouched for in regard to safety in the presence of gas and coal dust, when used in a proper way), some explosive which will have the same strength, and yet which, because of lessened percussive force or shattering effect, will produce coal in the manner desired. If one takes the other extreme, and considers a mine in which the product is used exclusively for the preparation of coke (and therefore where shattering of the coal is in no way a disadvantage), the mine superintendent's interest will be primarily to select an explosive which, as indicated by suitable lead-block, Trauzl, and ballistic pendulum tests, will produce the greatest amount of coal at the least cost.

As the cost of the explosive does not form any part of the tables prepared by the Testing Station, the relative cost must be computed from the manufacturer's prices, but the results tabulated by the Station will contain all the other data necessary to give the mine superintendent (who cares to take the small amount of trouble necessary to familiarize himself with the tables) all the information which is required to compare the action of one explosive with that of any other explosive tested.

In this way it is seen that, aside from the primary consideration of safety in the presence of explosive mixtures of fire-damp and coal dust (a condition alike fulfilled by all explosives admitted to the Permissible List), the data prepared by the Testing Station also give the information necessary to enable the discriminating mine manager to select an explosive adapted to the particular physical qualities of the coal at his mine, or to decide intelligently between two explosives of the same cost on the basis of their actual energy content

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in the particular form of the heaving or percussive force required in his work. Mr. Snelling.

Up to the present time the investigations have been confined to explosives used in coal mining, because the Act of Congress establishing the Testing Station has thus limited its work. Accordingly, it is not possible to compare, on the systematic basis just mentioned, the explosives generally used in rock work. It is probable that, if the Bill now before Congress in regard to the establishment of a Bureau of Mines is passed, work of this character will be undertaken, and the tables of explosives now prepared will be extended to cover all those intended for general mining and quarrying use. Data of such character are unobtainable to-day, and, as a result, a considerable percentage of explosives now used in all mining operations is wasted, because of their lack of adaptation to the materials being blasted. It is well known, for example, that when an explosive of high percussive force is used in excavating in a soft or easily compressed medium, a considerable percentage of its force is wasted as heat energy, performing no other function than the distortion and compression of the material in which it is fired, without exerting either an appreciable cracking or fissuring effect, or a heaving or throwing of the material.

Owing to lack of information in regard to the exact relationship between the percussive and the heaving force in particular explosives, this waste, as compared with the quantity required for the work with a properly balanced material, will continue; but it is to be hoped that it will soon be possible to give the mining and quarrying industries suitable information in regard to the properties of the various explosives, so that the railroad contractor and the metal miner may have the same simple and exact means of discrimination between suitable and unsuitable explosives that is now being provided for the benefit of the coal miner.

Another of the important but indirect benefits of this work has been the production of uniformity of strength and composition in explosives. An example of this helpful influence is the standardization of detonating caps and electric detonators. In the early days of the explosive industry, it was apparently advantageous for each manufacturer to have a separate system of trade nomenclature by which to designate the strengths of the different detonators manufactured by him. The necessity and even the advantage of such methods have long been outgrown, and yet, until the past year, the explosive industry has had to labor under conditions which made it almost impossible for the user of explosives to compare, in cost or strength, detonators of different manufacturers; or to select intelligently the detonator best suited to the explosive to be used. After conference with the manufacturers of detonating caps and electric detonators, a standard system of naming the strengths of these products has been selected by the

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Mr. Snelling.

Testing Station, and has met with a most hearty response. It is encouraging to note that, in recent trade catalogues, detonators are named in such a way as to enable the user to determine directly the strength of the contained charge, which is a decided advantage to every user of explosives and also to manufacturers.

The uniformity of composition of explosives (and many difficulties in mining work and many accidents have been rightly or wrongly attributed to lack of uniformity) may be considered as settled in regard to all those on the Permissible List. One of the conditions required of every explosive on that list is that its composition must continue substantially the same as the samples submitted originally for official test. Up to the present, all explosives admitted to the Permissible List have maintained their original composition, as determined by subsequent analyses of samples selected from mines in which the explosive was in use, and comparison with the original samples.

The data assembled by the Testing Station in regard to particular explosives have also been of great benefit to the manufacturers. When the explosives tests were commenced, comparatively few explosives were being made in the United States for which it was even claimed by the manufacturers that they were at all safe in the presence of explosive mixtures of gas or coal dust. It was evident that, without systematic tests, very little knowledge of the safety or lack of safety of any particular explosive could ever be gained, and, consequently, the user of explosives was apt to regard with incredulity any claim by the manufacturer in regard to the qualities of safety. Owing to lack of proof, this was most natural; and it was also evident that the very slow process of testing, which was offered by a study of mine explosions during past years, was sufficient only to prove the danger of black powder, and not in any way to indicate the safety of any of the brands of mining powder for which this property was claimed. Indeed, one of the few explosives to which the name, "safety," was attached, at the time the Government experiments were first undertaken, was found to be anything but safe when tested in the gallery, although there is no reason to believe that the makers of this and other explosives claiming "safety" for their product, did not have the fullest confidence in their safety.

The Testing Station offered the first opportunity in the United States to obtain facts in regard to the danger of any particular explosive in the presence of explosive mixtures of gas or coal dust. With most commendable energy, the manufacturers of explosives, noting the early failures of their powders in the testing gallery, began at once to modify them in such ways as suggested by the behavior of the explosives when under test, and, in a short time, returned to the Testing Station with improved products, able to stand the severe

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Mr. Snelling.

tests required. In this way the Testing Station has been a most active agent in increasing the general safety of explosives, and the manufacturers have shown clearly that it never was their desire to offer inferior explosives to the public, but that their failures in the past were due solely to lack of information in regard to the action of explosives under the conditions which exist before a mine disaster. The chance being offered to duplicate, at the Testing Station, the conditions represented in a mine in the presence of gas, they showed an eagerness to modify and improve their explosives so as to enable them to answer severe mining conditions, which is most commendable to American industry.

In regard to the unfavorable conditions existing in mines in the past, the same arguments may be used. In spite of the frequency of mine accidents in the United States, and in spite of the high death rate in coal mining as compared with that in other countries, it must be said in fairness that this has been the result of ignorance of the actual conditions which produce mine explosions, rather than any willful disregard of the known laws of safety by mine owners. Conditions in American mines are far different from those obtaining in mines abroad, and, as a result, the rules which years of experience had taught to foreign colliery managers were not quickly applied to conditions existing in American mines; but, as soon as the work at the Pittsburg Station had demonstrated the explosibility of the coal dust from adjoining mines, and had shown the very great safety of some explosives as compared with others, there was at once a readiness on the part of mine owners throughout the country to improve conditions in their mines, and to take advantage of all the studies made by the Government, thus showing clearly that the disasters of the past had been due to lack of sufficient information rather that to any willful disregard of the value of human lives.

Another of the indirect benefits of the work of the Station has resulted from its examination of explosives for the Panama Canal. For several years the Isthmian Canal Commission has been one of the largest users of explosives in the world, and, in the purchase of the enormous quantities required, it was found necessary to establish a system of careful examination and inspection. This was done in order to insure the safety of the explosives delivered on the Isthmus, and also to make certain that the standards named in the contract were being maintained at all times. With its established corps of chemists and engineers, it was natural that this important work should be taken up by the Technologic Branch of the United States Geological Survey, and, during the past three years, many millions of pounds of dynamite have been inspected and samples analyzed by the chemists connected with the Pittsburg Testing Station, thus insuring the high standard of these materials.

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Mr. Snelling.

One of the many ways in which this work for the Canal Commission has proved of advantage is shown by the fact that, as a result of studies at the Testing Station, electric detonators are being made to-day which, in water-proof qualities, are greatly superior to any similar product. As the improvements of these detonators were made by a member of the testing staff, all the pecuniary advantages arising from them have gone directly to the Government, which to-day is obtaining superior electric detonators, and at a cost of about one-third of the price of the former materials.

All the work of the Technologic Branch is being carried out along eminently practical lines, and is far removed from such work as can be taken up advantageously by private or by State agencies. The work of the Mine Accidents Division was taken up primarily to reduce the number of mine accidents, and to increase the general conditions of safety in mining. As the work of this Division has progressed, it has been found to be of great advantage to the miner and the mine owner, while the ultimate results of the studies will be of still greater value to every consumer of coal, as they will insure a continued supply of this valuable product, and at a lower cost than if the present methods, wasteful alike in lives and in coal, had been allowed to continue for another decade.

Mr. Bartoccini.

A. Bartoccini, Assoc. M. Am. Soc. C. E. (by letter).—The writer made a personal investigation of the mine disaster of Cherry, Ill. He interviewed the men who escaped on the day of the accident, and also several of those who were rescued one week later. He also interrogated the superintendent and the engineer of the mine, and obtained all the information asked for and also the plans of the mine showing the progress of the work.

After a careful investigation the writer found that the following conditions existed at the mine at the time of the disaster:

First.—There were no means for extinguishing fires in the mine.

Second.—There were no signal systems of any kind. Had the mine been provided with electric signals and telephones, like some of the most modern mines in the United States, the majority of the men could have been saved, by getting into communication with the outside and working in conjunction with the rescuers.

Third.—The miners had never received instructions of how to behave in case of fire.

Fourth.—The main entries and stables were lighted with open torches.

Fifth.—The organization of the mine was defective in some way, for at the time of the disaster orders came from every direction.

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Sixth.—The air shaft was used also as a hoisting shaft. Mr. Bartoccini.

Seventh.—The main shaft practically reached only to the second vein; its extension to the third and deepest vein was not used.

Eighth.—Plans of the workings of the second and third veins were not up to date. The last survey recorded on them was that of June, 1909. This would have made rescue work almost impossible to men not familiar with the mine.

Ninth.—The inside survey of the mine was not connected with the outside survey.

Would it not be possible for the United States Geological Survey to enforce rules which would prevent the existence of conditions such as those mentioned? The Survey is doing wonderful work, as shown by the rescue of twenty miners at Cherry one week after the conflagration; but there is no doubt that perhaps all the men could have been saved if telephone communications with the outside had been established. Telephone lines to resist any kind of a fire, can easily be installed, and the expense is small, almost negligible when one considers the enormous losses suffered by the mine owners and by the families of the victims.

H. G. Stott, M. Am. Soc. C. E.—The curves shown by Mr. Wilson give a clear general idea of the relative efficiencies of steam and gas engines when treated from a purely theoretical thermodynamic point of view. This point of view, however, is only justified when small units having a maximum brake horse-power not exceeding 1 000 are considered. Mr. Stott.

The steam engine or turbine operating under a gauge pressure of 200 lb. per sq. in., and with 150° superheat, has a maximum temperature of 538° Fahr. in its cylinder, while that of the gas engine varies between 2 000° and 3 000° Fahr.

The lubrication of a surface continually subjected to the latter temperature would be impossible, so that water jackets on the cylinders and, in the larger units, in the pistons become absolutely necessary. As the cylinders increase in diameter, it is necessary, of course, to increase their strength in proportion to their area, which, in turn, is proportional to the square of the diameter. The cooling surface, however, is only proportional to the circumference, or a single function of the diameter. Increasing the strength in proportion to the square of the diameter soon leads to difficulties, because of the fact that the flow of heat through a metal is a comparatively slow process; the thick walls of the cylinders on large engines cannot conduct the heat away fast enough, and all sorts of strains are set up in the metal, due to the enormous difference in temperature between the inside and the jacket lining of the cylinder.

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Mr. Stott. These conditions produce cut and cracked cylinders, with a natural resultant of high maintenance and depreciation costs. These costs, in some cases, have been so great, not only in the United States, but in Europe and Africa, as to cause the complete abandonment of large gas engine plants after a few years of attempted operation.

The first consideration in any power plant is that it shall be thoroughly reliable in operation, and the second is that it shall be economical, not only in operation, but in maintenance and depreciation. Therefore, in using the comparative efficiency curves shown in Mr. Wilson's paper it should be kept in mind that the cost of power is not only the fuel cost, but the fuel plus the maintenance and depreciation charges, and that the latter items should not be taken from the first year's account, but as an average of at least five years.

The small gas engine is a very satisfactory apparatus when supplied with good, clean gas, and when given proper attention, but great caution should be used before investing in large units, until further developments in the art take place, as conservation of capital is just as important as conservation of coal.

Mr. Dunn. B. W. DUNN, ESQ.* (by letter.)—The growing importance of investigations of explosives, with a view to increasing the consumer's knowledge of proper methods for handling and using them, is evident when it is noted that the total production of explosives in the United States has grown from less than 9 000 000 lb. in 1840 to about 215 000-000 lb. in 1905. Table 5 has been compiled by the Bureau of Explosives of the American Railway Association.

TABLE 5.—MANUFACTURE OF EXPLOSIVES IN THE UNITED STATES, 1909.

Kind of explosives.	Number of factories.	Maximum Capacity, in Pounds.	
		Daily.	Annual.
Black powder	49	1 220 150	366 135 000
High explosives	37	1 208 935	361 180 500
Smokeless powders	5	75 686	22 705 800

The first problem presented by this phenomenal increase relates to the safe transportation of this material from the factories to points of consumption. A package of explosives may make many journeys through densely populated centers, and rest temporarily in many widely separated storehouses before it reaches its final destination. A comprehensive view of the entire railway mileage of the United States would show at any instant about 5 000 cars partially or completely loaded with explosives. More than 1 200 storage magazines are listed by the Bureau of Explosives as sources of shipments of explosives by rail.

* Lieutenant-Colonel, Ordnance Dept., U. S. A.

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The increase in the demand for explosives has not been due entirely to the increase in mining operations. The civil engineer has been expanding his use of them until now carloads of dynamite, used on the Isthmus of Panama in a single blast, bring to the steam shovels as much as 75 000 cu. yd. of material, the dislodgment of which by manual labor would have required days of time and hundreds of men. Without the assistance of explosives, the construction of subways and the driving of tunnels would be impracticable. Even the farmer has awakened to the value of this concentrated source of power, and he uses it for the cheap and effective uprooting of large stumps over extended areas in Oregon, while an entire acre of subsoil in South Carolina, too refractory for the plow, is broken up and made available for successful cultivation by one explosion of a series of well-placed charges of dynamite. It has also been found by experience that a few cents' worth of explosive will be as effective as a dollar's worth of manual labor in preparing holes for transplanting trees. Mr. Dunn.

The use of explosives in war and in preparation for war is now almost a negligible quantity when compared with the general demand from peaceful industries. With the completion of the Panama Canal, it is estimated that the Government will have used in that work alone more explosives than have been expended in all the battles of history.

Until a few years ago little interest was manifested by the public in safeguarding the manufacture, transportation, storage, and use of explosives. Anyone possessing the necessary degree of ignorance, or rashness, was free to engage in their manufacture with incomplete equipment; they were transported by many railroads without any special precautions; the location of magazines in the immediate vicinity of dwellings, railways, and public highways, was criticized only after some disastrous explosion; and the often inexperienced consumer was without access to a competent and disinterested source of information such as he now has in the testing plant at Pittsburg so well described by Mr. Wilson.

The first general move to improve these conditions is believed to have been made by the American Railway Association in April, 1905. It resulted in the organization of a Bureau of Explosives which, through its inspectors, now exercises supervision over the transportation of all kinds of dangerous articles on 223 630 of the 245 000 miles of railways in the United States and Canada. A general idea of the kind and volume of inspection work is shown by the following extracts from the Annual Report of the Chief Inspector, dated February, 1910:

	1909.	*1908.*
"Total number of railway lines members of Bureau December 31st	172	158
Total mileage of Bureau lines December 31st	209 984	202 186
Total number of inspections of stations for explosives	6 953	5 603
Number of stations receiving two or more inspections for explosives	1 839	1 309

Mr. Dunn.

	1909.	1908.
Total number of inspections of stations for inflammables	6 950	1 098
Number of stations receiving two or more inspections for inflammables	1 886	....
Total number of inspections of factories	278	270
Number of factories receiving two or more inspections	75	69
Total number of inspections of magazines	1 293	1 540
Number of magazines receiving two or more inspections	349	361
Total number of boxes of high explosives condemned as unsafe for transportation	10 029	4 852
Total number of kegs of black powder condemned as unsafe for transportation	1 468	531
Total number of cars in transit containing explosives inspected	475	448
Total number of cars in transit showing serious violations of the regulations	168	197
Total number of inspections of steamship companies' piers (inflammable, 75; explosive, 63)	138	....
Total number of inspections made by Bureau	16 087	8 959
Total number of lectures to railway officials and employes and meetings addressed on the subject of safe transportation of explosives and other dangerous articles	215	171

	1909.	1908.	1907.
"Total number of accidents resulting in explosions or fires in transportation of explosives by rail	12	22	79
Total known property loss account explosions or accidents in transporting explosives by rail	$2 673	$114 629	$496 820
Total number of persons injured by explosions in transit	7	53	80
Total number of persons killed by explosions in transit	6	26	52

"During the same period reports have been rendered to the Chief Inspector by the Chemical Laboratory of the Bureau on 734 samples, as follows:

Explosives	211
Fireworks	186
Inflammables	304
Paper for lining high explosive boxes	31
Ammunition	2
Total	734

"As a means of ensuring the uniform enforcement of the regulations, by a well grounded appreciation of their significance and application, the lectures delivered by representatives of the Bureau have

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proved most successful. The promulgation of the regulations is not of itself sufficient to ensure uniformity or efficiency in their observance, and so these lectures form a valuable supplement to the inspection service. They have been successfully continued throughout the year, and the requests for the delivery of them by the managements of so many of the membership lines, is a convincing testimonial of the high esteem in which they are held. Mr. Dunn.

"While the lectures are primarily intended for the instruction and information of the officials and employes of the railway companies, and especially of those whose duties bring them into immediate contact with the dangerous articles handled in transportation, the manufacturers and shippers are invited, and they have attended them in considerable numbers. Many of this class have voluntarily expressed their commendation of the lectures as a medium of education, and signified their approval of them in flattering terms.

"The scope of these lectures embraces elementary instruction in the characteristics of explosives and inflammables and the hazards encountered in their transportation and in what respects the regulations afford protection against them. The requirements of the law, and the attendant penalties for violation, are fully described. Methods of preparation, packing, marking, receiving, handling and delivering, are explained by stereopticon lantern slides. These are interesting of themselves, and are the best means of stamping the impression they are intended to convey upon the minds of the audiences, and are always an acceptable feature of the lectures. The reception generally given to the lectures by those who have attended them, often at the voluntary surrender of time intended for rest while off duty, may be stated as an indication that the subject matter is one in which they are interested.

"The facilities of the Young Men's Christian Association, in halls, lanterns and skilled lantern operators, have been generously accorded and made use of to great advantage, in connection with the lectures at many places. The co-operation of this Association affords a convenient and economical method of securing the above facilities, and the Association has expressed its satisfaction with the arrangement as in line with the educational features which they provide for their members.

"During the year 1909, 215 lectures were delivered at various points throughout the United States."

The Bureau of Explosives, of the American Railway Association, and the Bureau of Mines, of the United States Geological Survey, were independent products of a general agitation due to the appreciation by a limited number of public-spirited citizens of the gravity of the "explosive" problem. It is the plain duty of the average citizen to become familiar with work of this kind prosecuted in his behalf. He may be able to help the work by assisting to overcome misguided opposition to it. Evidences of this opposition may be noted in the efforts of some shippers to avoid the expense of providing suitable shipping containers for explosives and inflammable articles, and in the threats of miners' labor unions to strike rather than use permissible explosives instead of black powder in mining coal in gaseous or dusty mines.

Too much credit cannot be given Messrs. Holmes and Wilson, and

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Mr. Dunn. other officials of the Technologic Branch of the United States Geological Survey, for the investigations described in this paper. They are establishing reasonable standards for many structural materials; they are teaching the manufacturer what he can and should produce, and the consumer what he has a right to demand; with scientific accuracy they are pointing the way to a conservation of our natural resources and to a saving of life which will repay the nation many times for the cost of their work.

When these facts become thoroughly appreciated and digested by the average citizen, these gentlemen and their able assistants will have no further cause to fear the withdrawal of financial or moral support for their work.

Mr. Wilson. HERBERT M. WILSON, M. AM. SOC. C. E. (by letter).—The Fuel Division of the United States Geological Survey has given considerable attention to the use of peat as a fuel for combustion under boiler furnaces, in gas producers, and for other purposes. It is doubtless to this material that Mr. Allen refers in speaking of utilizing "marsh mud for fuel," since he refers to an address by Mr. Edward Atkinson on the subject of "Bog Fuel" in which he characterized peat by the more popular term "marsh mud."

In Europe, where fuel is expensive, 10 000 000 tons of peat are used annually for fuel purposes. A preliminary and incomplete examination, made by Mr. C. A. Davis, of the Fuel Division of the Geological Survey, indicates that the peat beds of the United States extend throughout an area of more than 11 000 sq. miles. The larger part of this is in New England, New York, Minnesota, Wisconsin, New Jersey, Virginia, and other Coastal States which contain little or no coal. It has been estimated that this area will produce 13 000 000 000 tons of air-dried peat.

At present peat production is in its infancy in the United States, though there are in operation several commercial plants which find a ready market for their product and are being operated at a profit. A test was made at the Pittsburg plant on North Carolina peat operated in a gas producer—the resulting producer gas being used to run a gas engine of 150 h.p.—the load on which was measured on a switch-board. Peat containing nearly 30% of ash and 15% of water gave 1 commercial horse-power-hour for each 4 lb. of peat fired in the producer. Had the peat cost $2 per ton to dig and prepare for the producer, each horse-power-hour developed would have cost 0.4 of a cent. The fuel cost of running an electric plant properly equipped for using peat fuel, of even this low grade, in the gas producer would be about $4 per 100 h.p. developed per 10-hour day.

Equally good results were procured in tests of Florida and Michigan peat operated in the gas producer. The investigations of peat under Mr. Davis include studies of simple commercial methods of drying, the chemical and fuel value, analyses of the peat, studies of the

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mechanical methods of digging and disintegrating the peat, and physical tests to determine the strength of air-dried peat to support a load. Mr. Wilson.

The calorific value of peat, as shown by numerous analyses made by the United States Geological Survey, runs from about 7 500 to nearly 11 000 B.t.u., moisture free, including the ash, which varies from less than 2% to 20%, the latter being considered in Europe the limit of commercial use for fuel. Analyses of 25 samples of peat from Florida, within these limits as to ash, show a range of from 8 269 to 10 865 B.t.u., only four of the series being below 9 000 B.t.u., and four exceeding 10 500 B.t.u., moisture free. Such fuel in Florida is likely to be utilized soon, since it only needs to be dug and dried in order to render it fit for the furnace or gas producer. Many bituminous coals now used commercially have fuel value as low as 11 000 B.t.u., moisture free, and with maximum ash content of 20%; buckwheat anthracite averages near the same figures, often running as high as 24% ash.

One bulletin concerning the peats of Maine has been published, and another, concerning the peat industries of the United States, is in course of publication.

Mr. Bartoccini asks whether it would not be possible for the United States Geological Survey to enforce rules which would prevent the existence of conditions such as occurred at the mine disaster of Cherry, Ill.

The United States Government has no police power within the States, and it is not within its province to enact or enforce rules or laws, or even to make police inspection regarding the methods of operating mining properties. The province of the mine accidents investigations and that of its successor, the Bureau of Mines, is, within the States, like that of other and similar Government bureaus in the Interior Department, the Department of Agriculture, and other Federal departments, merely to investigate and disseminate information. It remains for the States to enact laws and rules applying the remedies which may be indicated as a result of Federal investigation.

Investigations are now in progress and tests are being conducted with a view to issuing circulars concerning the methods of fighting mine fires, the installation of telephones and other means of signaling, and other subjects of the kind to which Mr. Bartoccini refers.

Much as the writer appreciates the kindly and sympathetic spirit of the discussion of Messrs. Allen and Bartoccini, he appreciates even more that of Colonel Dunn and Mr. Stott, who are recognized authorities regarding the subjects they discuss, and of Messrs. Kreisinger and Snelling, who have added materially to the details presented in the paper relative to the particular investigations of which they have charge in Pittsburg.

Mr. Snelling's reference to the use of explosives in blasting opera-

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Mr. Wilson. tions should be of interest to all civil engineers, as well as to mining engineers, as should Colonel Dunn's discussion concerning the means adopted to safeguard the transportation of explosives.

Since the presentation of the paper, Congress has enacted a law establishing, in the Department of the Interior, a United States Bureau of Mines. To this Bureau have been transferred from the Geological Survey the fuel-testing and the mine accidents investigations described in this paper. To the writer it seems a matter for deep regret that the investigations of the structural materials belonging to and for the use of the United States, were not also transferred to the same Bureau. On the last day of the session of Congress, a conference report transferred these from the Geological Survey to the Bureau of Standards. It is doubtful whether the continuation of these investigations in that Bureau, presided over as it is by physicists and chemists of high scientific attainments, will be of as immediate value to engineers and to those engaged in building and engineering construction as they would in the Bureau of Mines, charged as it is with the investigations pertinent to the mining and quarrying industries, and having in its employ mining, mechanical, and civil engineers.

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AMERICAN SOCIETY OF CIVIL ENGINEERS

INSTITUTED 1852

TRANSACTIONS

Paper No. 1172

LOCOMOTIVE PERFORMANCE ON GRADES OF VARIOUS LENGTHS.

By Beverly S. Randolph, M. Am. Soc. C. E.

With Discussion by Messrs. C. D. Purdon, John C. Trautwine, Jr., and Beverly S. Randolph.

In the location of new railways and the improvement of lines already in operation, it is now well recognized that large economies can be effected by the careful study of train resistance due to grades and alignment, distributing this resistance so as to secure a minimum cost of operation with the means available for construction.

While engaged in such studies some years ago, the attention of the writer was attracted by the fact that the usual method of calculating the traction of a locomotive—by assuming from 20 to 25% of the weight on the drivers—was subject to no small modification in practice.

In order to obtain a working basis, for use in relation to this feature, he undertook the collection of data from the practical operation of various roads. Subsequent engagements in an entirely different direction caused this to be laid aside until the present time. The results are given in Table 1, from which it will be seen that the percentage of driver weight utilized in draft is a function of the length as well as the rate of grade encountered in the practical operation of railways.

In this table, performance will be found expressed as the percentage of the weight on the drivers which is utilized in draft. This is calculated on a basis of 6 lb. per ton of train resistance, for dates

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prior to 1880, this being the amount given by the late A. M. Wellington, M. Am. Soc. C. E.,* and 4.7 lb. per ton for those of 1908-10, as obtained by A. C. Dennis, M. Am. Soc. C. E.,† assuming this difference to represent the advance in practice from 1880 to the present time. Most of the data have been obtained from the "Catalogue of the Baldwin Locomotive Works" for 1881, to which have been added some later figures from "Record No. 65" of the same establishment, and also some obtained by the writer directly from the roads concerned. Being taken thus at random, the results may be accepted as fairly representative of American practice.

Attention should be directed to the fact that the performance of the 10-34 E, Consolidation locomotive on the Lehigh Valley Railroad in 1871 is practically equal to that of the latest Mallet compounds on the Great Northern Railway. In other words, in the ratio between the ability to produce steam and the weight on the drivers there has been no change in the last forty years. This would indicate that the figures are not likely to be changed much as long as steam-driven locomotives are in use. What will obtain with the introduction of electric traction is "another story."

These results have also been platted, and are presented in Fig. 1, with the lengths of grade as abscissas and the percentages of weight utilized as ordinates. The curve sketched to represent a general average will show the conditions at a glance. The results may at first sight seem irregular, but the agreement is really remarkable when the variety of sources is considered; that in many cases the "reputed" rate of grade is doubtless given without actual measurement; that the results also include momentum, the ability to utilize which depends on the conditions of grade, alignment, and operating practice which obtain about the foot of each grade; and that the same amount of energy due to momentum will carry a train farther on a light grade than on a heavy one.

There are four items in Table 1 which vary materially from the general consensus. For Item 9, the authorities of the road particularly state that their loads are light, because, owing to the congested condition of their business, their trains must make fast time. Item 10 represents very old practice, certainly prior to 1882, and is "second-

* "The Economic Theory of Railway Location," 1887 edition, p 502.

† *Transactions*, Am. Soc. C. E., Vol. L, p. 1.

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hand." The load consisted of empty coal cars, and the line was very tortuous, so that it is quite probable that the resistance assumed in the calculation is far below the actual. Items 15 and 17 are both high. To account for this, it is to be noted that this road has been recently completed, regardless of cost in the matter of both track and rolling stock, and doubtless represents the highest development of railroad practice. Its rolling stock is all new, and is probably in better condition to offer low resistance than it will ever be again, and

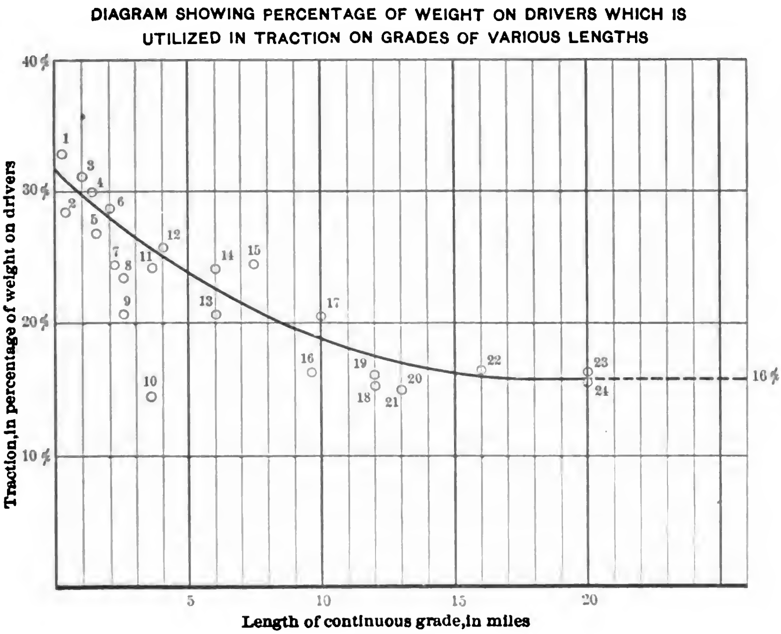

FIG. 1.

there were no "foreign" cars in the trains considered. The train resistance, therefore, may be naturally assumed to be much less than that of roads hauling all classes of cars, many of which are barely good enough to pass inspection. As the grades are light in both cases, this feature of train resistance is larger than in items including heavier grades. Attention should be called to the fact that a line connecting the two points representing these items on Fig. 1 would make only a small angle with the sketched curve, and would be practically parallel to a similar line connecting the points represented

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by Items 13 and 16. There is, therefore, an agreement of ratios, which is all that needs consideration in this discussion.

Wellington, in his monumental work on railway location, presents a table of this character. The percentages of weight on the drivers which is utilized in draft show the greatest irregularity. He does not give the length of the grades considered, so that it is impossible to say how far the introduction of this feature would have contributed to bring order out of the chaos. In his discussion of the table he admits the unsatisfactory character of the results, and finally decides on 25% as a rough average, "very approximately the safe operating load in regular service." He further states that a number of results, which he omits for want of space, exceeds 33 per cent. The highest shown in Table 1 will be found in Item 1 (0.06 mile, 0.066 grade), showing 33 per cent. There is no momentum effect here, as the grade is a short incline extending down to the river, and the start is necessarily a "dead" one. The reports of Item 3, which shows 31%, and Item 5, which shows 27%, state specifically that the locomotives will stop and start the loads given at any point on the grade.

The results of a series of experiments reported by Mr. A. C. Dennis in his paper, "Virtual Grades for Freight Trains," previously referred to, indicate a utilization of somewhat more than 23%, decreasing with the speed.

All this indicates that the general failure of locomotives to utilize more than from 16 to 18% on long grades, as shown by Table 1, can only be due to the failure of the boilers to supply the necessary steam. While the higher percentage shown for the shorter grades may be ascribed largely to momentum present when the foot of the grade is reached, the energy due to stored heat is responsible for a large portion of it.

When a locomotive has been standing still, or running with the steam consumption materially below the production, the pressure accumulates until it reaches the point at which the safety valve is "set." This means that the entire machine is heated to a temperature sufficient to maintain this pressure in the boiler. When the steam consumption begins to exceed the production, this temperature is reduced to a point where the consumption and production balance.

The heat represented by this difference in temperature has passed into the steam used, thus adding to the energy supplied by the com-

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bustion going on in the furnace. The engines, therefore, are able to do considerably more work during the time the pressure is falling than they can do after the fall has ceased.

The curve in Fig. 1 would indicate that the energy derived from the two sources just discussed is practically dissipated at 15 miles, though the position of the points representing Items 16, 18, 19, 20, and 21 would indicate that this takes place more frequently between 10 and 12 miles. From this point onward the performance depends on the efficiency of the steam production, which does not appear to be able to utilize more than 16% of the weight on the drivers. The diagrams presented by Mr. Dennis in his paper on virtual grades, and by John A. Fulton, M. Am. Soc. C. E., in his discussion of that paper, indicate that similar results would be shown were they extended to include the distance named.

From this it would appear that a locomotive is capable of hauling a larger train on grades less than 10 miles in length than on longer grades, and that, even when unexpectedly stopped, it is capable of starting again as soon as the steam pressure is sufficiently built up. Conversely, it should be practicable to use a higher rate of ascent on shorter grades on any given line without decreasing the load which can be hauled over it. In other words, what is known as the "ruling grade" is a function, strictly speaking, of the length as well as the rate of grade.

In any discussions of the practicability of using a higher rate on the short grades, which the writer has seen, the most valid objection has appeared to be the danger of stalling and consequent delay. As far as momentum is relied on, this objection is valid. Within the limits of the load which can be handled by the steam, it has small value, as it is only a question of waiting a few minutes until the pressure can be built up to the point at which the load can be handled. As this need only be an occasional occurrence, it is not to be balanced against any material saving in cost of construction.

The writer does not know of any experiments which will throw much light on the value of heat storage as separated from momentum, though the following discussion may prove suggestive:

A train moving at a rate of 60 ft. per sec., and reaching the foot of a grade, will have acquired a "velocity head" of 56.7 ft., equivalent to stored energy of $56.7 \times 2\,000 = 113\,400$ ft-lb. per ton. On a 0.002

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Reporting Officer.	Year.	Source of Data.	Remarks.
Newell Tilton, Asst. Supt.	1880	Baldwin Catalogue, 1881, p. 184	
S. Spencer, Gen. Supt.	1878	" " 1881, " 72	10 miles per hour.
J. D. Burr, Asst. Engr.	1879	" " 1881, " 115	8 " " " Stops and starts on grade.
J. E. Martin, Local Supt.	1879	" " 1881, " 100	
H. B. Stone	1880	" " 1881, " 116	Stops and starts at any point on grade.
"	1880	" " 1881, " 116	
"	1880	" " 1881, " 116	
C. W. Rogers, Gen. Mgr.	1879	" " 1881, " 87	
....	1910	....	
....	....	Trautwine's Pocket Book. Ed. 1882, p. 412..................	Empty cars; many curves and reversions.
J. D. Burr, Asst. Engr.	1879	Baldwin Catalogue, 1881, p. 114	
John Hewitt, Supt. M. P.	1880	" " 1881, " 112	
D. Holtz, M. of Mach'y.	1878	" " 1881, " 86	12 miles per hour.
J. D. Burr, Asst. Engr.	1879	" " 1881, " 114	8 " " "
....	1910	*Engineering News*, Jan. 13. 1910.	
....	....	Trautwine's Pocket Book, Ed. 1882, p. 412..................	
....	1910	*Engineering News*, Jan. 13, 1910.	Road locomotive and helper.
A. Mitchell, Div. Supt.	1871	Baldwin Catalogue, 1881, p. 112.	
Grafton Greenough.	1908	Baldwin Loco. Wks. Record, No. 65, p. 29..................	
Grafton Greenough.	1908	Baldwin Loco. Wks. Record. No. 65, p. 23.................	
F. E. Blaser, Div. Supt.	1910	....	Very crooked line. Uncompensated.
W. W. Stearns, Asst.Gen.Supt.	1880	Baldwin Catalogue, 1881, p. 113.	
Grafton Greenough.	1908	Baldwin Loco. Wks. Record, No. 65, p. 29..................	
Grafton Greenough	1906	Baldwin Loco. Wks. Record, No. 65, p. 23..................	

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grade (as in Item 15 of Table 1) the resistance would be, gravity 4 lb. + train 4.7 lb. = 8.7 lb., against which the energy above given would carry the train through $\frac{113\,400}{8.7} = 13\,034$ ft., say, 2.5 miles, leaving 5 miles to be provided for by the steam production. Examining the items in the table having grades in excess of 10 miles, it will be noted that 16% is about all the weight on drivers which can be utilized by the current supply of steam. In Item 15 the energy derived from all sources is equivalent to 24.3%; hence the stored heat may be considered as responsible for an equivalent of 24.3% — 16% = 8.3% for a distance of 5 miles.

In proportioning grade resistance for any line, therefore, a locomotive may be counted on to utilize 24.3% of the weight on the drivers for a distance of 5 miles on a 0.002 grade without any assistance from momentum, and, in the event of an unexpected stop, should be able, as soon as a full head of steam is built up, to start the train and carry it over the grade. This is probably a maximum, considering the condition of the equipment of this Virginian Railway, as previously mentioned.

Treating Item 14 in the same way, a distance of 2 310 ft. is accounted for by momentum, leaving, say, 5.5 miles for the steam, or the length of a 0.02 grade on which a locomotive may be loaded on a basis of tractive power equal to 24.2% of the weight on the drivers.

From these figures it may be concluded that on lines having grades from 12 to 15 or more miles in length, grades of 3 to 5 miles in length may be inserted having rates 50% in excess of that of the long grades, without decreasing the capacity of the line. This statement, of course, is general in its bearings, each case being subject to its especial limitations, and subject to detailed calculations.

It may be noted that the velocity of 60 ft. per sec., assumed at the foot of the grade, is probably higher than should be expected in practice; it insures, on the other hand, that quite enough has been allowed for momentum, and that the results are conservative.

Arguments like the foregoing are always more or less treacherous; being based on statistics, they are naturally subject to material modifications in the presence of a larger array of data, therefore, material assistance in reaching practical conclusions can be given by the presentation of additional data.

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DISCUSSION

C. D. PURDON, M. AM. SOC. C. E. (by letter).—Some years ago the writer, in making studies for grade revision, found that the tractive power of a locomotive up grade becomes less as the length of the grade increases, and in some unknown proportion. This was a practical confirmation of the saying of locomotive engineers, that the engine "got tired" on long grades. On a well-known Western railroad, with which the writer is familiar, experiments were made for the purpose of rating its locomotives. The locomotives were first divided into classes according to their tractive power, this being calculated by the usual rule, with factors of size of cylinders, boiler pressure, and diameter of drivers, also by taking one-fourth of the weight on the drivers, and using the lesser of the two results as the tractive power. Mr. Purdon.

Locomotives of different classes, and hauling known loads, were run over a freight division, the cars being weighed for the purpose; thus the maximum load which could be handled over a division, or different parts of a division, was ascertained, and this proportion of tonnage to tractive power was used in rating all classes.

Of course, this method was not mathematically accurate, as the condition of track, the weather, and the personal equation of the locomotive engineers all had an effect, but, later, when correcting the rating by tests with dynamometers, it was found that the results were fairly practical.

There were three hills where the rate of grade was the same as the rest of the division, but where the length was much in excess of other grades of the same rate.

Designating these hills as *A*, *B*, and *C*, the lengths are, respectively, 2.44, 3.57, and 4.41 miles. There were no other grades of the same rate exceeding 1 mile.

In one class of freight engines, 10-wheel Brooks, the weight of the engine was 197 900 lb.; tender, 132 800 lb.; weight on drivers, 142 600 lb.; boiler pressure, 200 lb.; and tractive power of cylinders, 33 300 lb.

On Hill *A* these engines are rated at 865 tons, as compared with 945 on other parts of the division. As the engine weighs 165 tons and the caboose 15 tons, 180 tons should be added, making the figures, 1 045 and 1 125 tons. Thus the length of the grade, 2.44 miles, makes the tractive power on it 92% of that on shorter grades.

On Hill *B*, the rating, adding 180 tons as above, is 1 160 and 1 230 tons, respectively, giving 94% for 3.57 miles.

On Hill *C*, the rating, with 180 tons added, is 1 130 and 1 230 tons, making 92% for 4.41 miles.

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Mr. Purdon. Taking the same basis as the author, namely, 4.7 lb. per ton, rate of grade × 20, and weight on drivers, gives:

Hill *A*, 18.078%,	remainder of division,	19.462%
Hill *B*, 20.068%,	" " "	21.279%
Hill *C*, 19.549%,	" " "	21.279%

It will be noted that the author uses the weight on the drivers as the criterion, but the tractive power is not directly as the weight on the drivers, some engines being over-cylindered, or under-cylindered; in the class of engines above mentioned the tractive power is 23.35% of the weight on the drivers.

The writer made a study of several dynamometer tests on Hill *C*. There is a grade of the same rate, about 1 mile long, near this hill, and a station near its foot, but there is sufficient level grade between this station and the foot of the hill to get a good start.

All the engines of the above class, loaded for Hill *C*, gained speed on the 1-mile grade, but began to fall below the theoretical speed at a point about 2¼ miles from the foot of the hill. This condition occurred when the trains stopped at the station and also when they passed it at a rate of some 16 or 18 miles per hour, the speed becoming less and less as the top of the hill was approached.

The writer concludes that the author might stretch his opinion as to using heavier rates of grade on shorter hills than 10 miles, and indeed his diagram seems to intimate as much, and that, for economical operation, the maximum rate of grade should be reduced after a length of about 2 miles has been reached, and more and more in proportion to the length of the hill, in order that the same rating could be applied all over a division.

This conclusion might be modified by local conditions, such as an important town where cars might be added to or taken from the train.

While it does not seem practicable to the writer to calculate what the reduction of rate of grade should be, a consensus of results of operation on different lengths of grade might give sufficient data to reach some conclusion on the matter.

The American Railway Engineering and Maintenance of Way Association has a Committee on "Railway Economics," which is studying such matters, but so far as the writer knows it has not given this question any consideration.

The writer hopes that the author will follow up this subject, and that other members will join, as a full discussion will no doubt bring some results on a question which seems to be highly important.

Mr. Trautwine. JOHN C. TRAUTWINE, JR., ASSOC. AM. SOC. C. E. (by letter).—In his collection of data, Mr. Randolph includes two ancient cases taken from the earliest editions (1872-1883) of Trautwine's "Civil

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Mr. Trautwine.

Engineer's Pocket-Book," referring to performances on the Mahanoy and Broad Mountain Railroad (now the Frackville Branch of the Reading) and on the Pennsylvania Railroad, respectively.

In the private notes of John C. Trautwine, Sr., these two cases are recorded as follows:

"On the Mahanoy & Broad Mtn. R. R., *tank* Engines of 35 tons, *all on 8 drivers,* draw 40 *empty* coal cars weighing 100 tons, *up* a continuous grade of 175 ft. per mile for $3\frac{1}{2}$ miles; & around curves of 450, 500, 600 ft. &c. rad., at 8 miles an hour. (1864) This is equal to $77\frac{14}{100}$ tons for a 27-ton engine." (Vol. III, p. 176.)

"On the Penn Central 95 ft. grades for $9\frac{3}{4}$ miles, a 29-ton engine all on 8 drivers takes 125 tons of freight and 112 tons of engine, tender, & cars, in all 237 tons,* and a passenger engine takes up 3 cars at 24 miles an hour (large 8 wheels). When more than 3, an auxiliary engine."

It will be seen that Mr. Randolph is well within bounds in ascribing to the Mahanoy and Broad Mountain case (his No. 10) a date "certainly prior to 1882," the date being given, in the notes, as 1864; while another entry just below it, for the Pennsylvania Railroad case, is dated 1860.

It also seems, as stated by Mr. Randolph, quite probable that the frictional resistance (6 lb. per 2 000 lb.) assumed by him in the calculation is far below the actual for this Case 10. The small, empty, four-wheel cars weighed only 4 400 lb. each. Furthermore, the "tons," in the Trautwine reports of these experiments, were tons of 2 240 lb. On the other hand, the maximum curvature was 12° 45′ (not 14°, as given by the author), and the engine was a tank locomotive, whereas the author has credited it with a 25-ton tender.

After making all corrections, it will be found that, in order to bring the point, for this Case 10, up to the author's curve, instead of his 6 lb. per 2 000 lb., a frictional resistance of 66 lb. per 2 000 lb. would be required, a resistance just equal to the gravity resistance on the 3.3% grade, making a total resistance of 132 lb. per 2 000 lb.

While this 66 lb. per ton is very high, it is perhaps not too high for the known conditions, as above described. For modern rolling stock, Mr. A. K. Shurtleff gives the formula:†

$$\left.\begin{array}{r}\text{Frictional resistance, on tangent,}\\ \text{in pounds per 2 000 pounds}\ldots\end{array}\right\} = 1 + \frac{90}{C},$$

where C = weight of car and load, in tons of 2 000 lb. This would give, for 4 400-lb. (2.2-ton) cars, a frictional resistance of 42 lb. per 2 000 lb.; and, on the usual assumption of 0.8 lb. per 2 000 lb. for each

* "Nearly 200 tons *exclusive* of eng. & ten." (Vol. III, p. $176\frac{1}{10}$.)

† American Railway Engineering and Maintenance of Way Association, Bulletin 84, February, 1907, p. 99.

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Mr. Trautwine.

degree of curvature, the 12.75° curves of this line would give 10 lb. per ton additional, making a total of 52 lb. per 2 000 lb. over and above grade resistance, under modern conditions.

In the 9th to 17th editions of Trautwine (1885-1900), these early accounts were superseded by numerous later instances, including some of those quoted by the author.

In the 18th and 19th editions (1902-1909) are given data respecting performances on the Catawissa Branch of the Reading (Shamokin Division) in 1898-1901. These give the maximum and minimum loads hauled up a nearly continuous grade of 31.47 ft. per mile (0.59%) from Catawissa to Lofty (34.03 miles) by engines of different classes, with different helpers and without helpers.

Table 2 (in which the writer follows the author in assuming frictional resistance at 4.7 lb. per 2 000 lb.) shows the cases giving the maximum and minimum values of the quantity represented by the ordinates in the author's diagram, namely, "Traction, in percentage of weight on drivers."

It will be seen that the maximum percentage (16.1) is practically identical with that found by the author (16) for grade lengths exceeding 17 miles.

Near the middle of the 34-mile distance there is a stretch of 1.51 miles, on which the average grade is only 5.93 ft. per mile (0.112%), and this stretch divides the remaining distance into two practically continuous grades, 19.39 and 13.13 miles long, respectively; but, as the same loads are hauled over these two portions by the same engines, the results are virtually identical, the maxima furnishing two more points closely coinciding with the author's diagram.

TABLE 2.—Tractive Force, Catawissa to Lofty.

Length of grade, in miles					34.03
Grade { in feet per mile					31.47
Grade { percentage				A	0.597
Resistances, in pounds per 2 000 lb., Gravity ($= 20\ A$) = 11.94. Friction = 4.70				B	16.64
Load:	Cars.	Locomotive.	Tender.		
Maximum*	1 561	44.60	25.25	C	1 631
Minimum†	1 031	60.50	34.50	C	1 126
Traction $\left(= \frac{B\ C}{2\ 000}\right)$ { Maximum*				D	13.60
Traction $\left(= \frac{B\ C}{2\ 000}\right)$ { Minimum†				D	9.38
Weight on Drivers:	Locomotive.	Helper.			
Maximum*	21.60	63.00		E	84.60
Minimum†	47.00	72.00		E	119.00
Percentage $\left(= \frac{D}{E}\right)$.					
Maximum				F	16.1
Minimum				F	7.9

*Giving maximum values of percentage, F.
†Giving minimum values of percentage, F.

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Mr. Randolph.

BEVERLY S. RANDOLPH, M. AM. SOC. C. E. (by letter).—The percentages given by Mr. Purdon would seem to indicate that the length of the grades did not affect the loads in the cases cited, but these percentages are so much below those shown in the table, for similar distances, as to indicate some special conditions which the writer has been unable to find in the text.

The use of the percentage of weight on drivers which is utilized in traction as a measure of the efficiency of the locomotive, while, probably, not applicable to individual machines, is sound for the purposes of comparison of results to be obtained on various portions of a line as far as affected by conditions of grade and alignment. It has the advantage of disregarding questions of temperature, condition of track, character of fuel, etc., which, being the same on all portions of the line, naturally balance and do not affect the comparison. It is, of course, simply a method of expressing the final efficiency of the various parts of the locomotive, and, since it depends entirely on actual results already accomplished, leaves no room for difference of opinion or theoretical error.

The writer has always considered an "under-cylindered" locomotive as a defective machine. All weight is a distinct debit, in the shape of wear and tear of track and running gear, resistance due to gravity on grades, interest on cost, etc. When this weight fails to earn a credit in the way of tractive efficiency, it should not be present.

The statement relative to the performance of locomotives on "Hill *C*" is interesting, especially in that it appears to have been immaterial whether they made a dead start after stopping at the station or approached the foot of the hill at 16 to 18 miles per hour. The momentum would appear to be an insignificant factor.

It is gratifying to note that Mr. Trautwine has been able to brace up the weak member of Table 1 so completely with his detailed data; also that his other results strengthen the conclusions reached in the paper.

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AMERICAN SOCIETY OF CIVIL ENGINEERS
INSTITUTED 1852

TRANSACTIONS

Paper No. 1173

A CONCRETE WATER TOWER.*

By A. Kempkey, Jr., Jun. Am. Soc. C. E.†

With Discussion by Messrs. Maurice C. Couchot, L. J. Mensch, A. H. Markwart, and A. Kempkey, Jr.

The City of Victoria is situated on the southern end of Vancouver Island, in the Province of British Columbia, Canada, and is the capital of the Province.

In common with all cities of the extreme West, its growth has been very rapid within the last few years. The population of the city proper, together with that of the municipality of Oak Bay, immediately adjacent, is now about 35 000.

The Victoria water-works are owned by the city and operated under the direction of a Water Commissioner appointed by the City Council. By special agreement, water is supplied to Oak Bay in bulk, this municipality having its own distributing system.

The rapid increase in population, together with the fact that in recent years very little had been done toward increasing the water supply, resulted in the necessity for remodeling the entire system, and there are very few cities where this would involve as many complex problems or a greater variety of work.

Water is drawn from Elk Lake, situated about five miles north of the city; thence it flows by gravity to the pumping station about four miles distant, and from there is pumped directly to the consumers.

* Presented at the meeting of March 16th, 1910.
† Now Assoc. M. Am. Soc. C. E.

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The remodeling of the system, as recently completed, provided for:

1.—Increasing the capacity of Elk Lake by a system of levees.

2.—Increasing the capacity of the main to the pumping station by replacing about two miles of the old 16-in., wrought-iron, riveted pipe with 24-in. riveted steel pipe.

3.—Increasing the capacity of the pumping station by the installation of a 4 500 000-gal. pumping engine of the close-connected, cross-compound, Corliss, crank-and-fly-wheel type.

4.—The construction of a 20 000 000-gal. concrete-lined distributing reservoir in the city.

5.—The entire remodeling of the distributing system, necessitating the laying of about ½ mile each of 18-in. and 27-in. pipe, and about 1 mile of 24-in. riveted steel pipe; also about 3 000 tons of cast-iron pipe, varying in size from 4 to 12 in.

6.—The provision for a high-level service by means of an elevated tank of approximately 100 000 gal. capacity, water being supplied to the tank by two electrically-driven triplex pumps, each having a capacity of 100 000 gal. per 24 hours, against a dynamic head of 150 ft., and arranged to start and stop automatically with a variation of 3 ft. in the elevation of the water in the tank. These pumps are located about one mile from the tower, and are controlled by a float-operated auto-start, in the base of the tower.

A description of the elevated tank, which is novel in design, with the reasons for adopting the type of structure used, the method of construction, and the detailed cost, form the basis of this paper.

The tower is on the top of the highest hill in the city, in the heart of the most exclusive residential district, beautiful homes clustering about its base. The necessity for architectural treatment of the structure is thus seen to be of prime importance. In fact, the opposition of the local residents to the ordinary type of elevated tank, that is, latticed columns supporting a tank with a hemispherical bottom and a conical roof, rendered its use impossible, although tenders were invited on such a structure.

It is believed that under the conditions of location, three types of structure should be considered: First, an all-steel structure, the ornamentation being produced by casing in with brick or concrete; second, a brick-and-steel, or a concrete-and-steel, structure, such as the one actually erected; third, a typical reinforced concrete structure.

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Considering only that portion below the tank, the amount of material required to case in a structure of the first type would be substantially the same as that used to support the tank in a structure of the second type. Consequently, the steel substructure, for all practical purposes, would represent a dead loss, and, therefore, the economy of this type is open to serious question.

A tender was received for a reinforced concrete structure identical in outward appearance with the one built, but, owing to the natural conservatism of the local residents regarding this type of construction, it was not acceptable.

The tower, as built, consists of a hollow cylinder of plain concrete, 109 ft. high, and having an inside diameter of 22 ft. The walls are 10 in. thick for the first 70 ft. and 6 in. thick for the remaining 39 ft., and are ornamented with six pilasters (70 ft. high, 3 ft. wide, and 7 in. thick), a 4-ft. belt, then twelve pilasters (12 ft. high, 18 in. wide, and 7 in. thick), a cornice, and a parapet wall.

A steel tank of the ordinary type is embedded in the upper 40 ft. of this cylinder. To form the bottom of this tank, a plain concrete dome is thrown across the cylinder at a point about 70 ft. from the base, the thrust of this dome being taken up by two steel rings, $\frac{1}{2}$ in. by 14 in. and $\frac{3}{8}$ in. by 18 in., bedded into the walls of the tower, the latter ring being riveted to the lower course of the tank.

The tank is covered with a roof of reinforced concrete, 4 in. thick, conical in shape, and reinforced with $\frac{1}{2}$-in. twisted steel bars. The design of the structure is clearly shown in Fig. 1.

The tower is built on out-cropping, solid rock. This rock was roughly stepped, and a concrete sub-base built. This sub-base consists of a hollow ring, with an inside diameter of 20 ft., the walls being 5 ft. thick. It is about 2 ft. high on one side and 7 ft. high on the other, and forms a level base on which the tower is built. The forms for this sub-base consist of vertical lagging and circumferential ribs. The lagging is of double-dressed, 2 by 3-in. segments, and the ribs are of 2 by 12-in. segments, 6 ft. long, lapping past one another and securely spiked together to form complete or partial circles. These ribs are 2 ft. from center to center.

Similar construction was used to form the taper base of the tower proper, except, of course, that the radii of the segments forming the successive ribs decreased with the height of the rib. Tapered lagging

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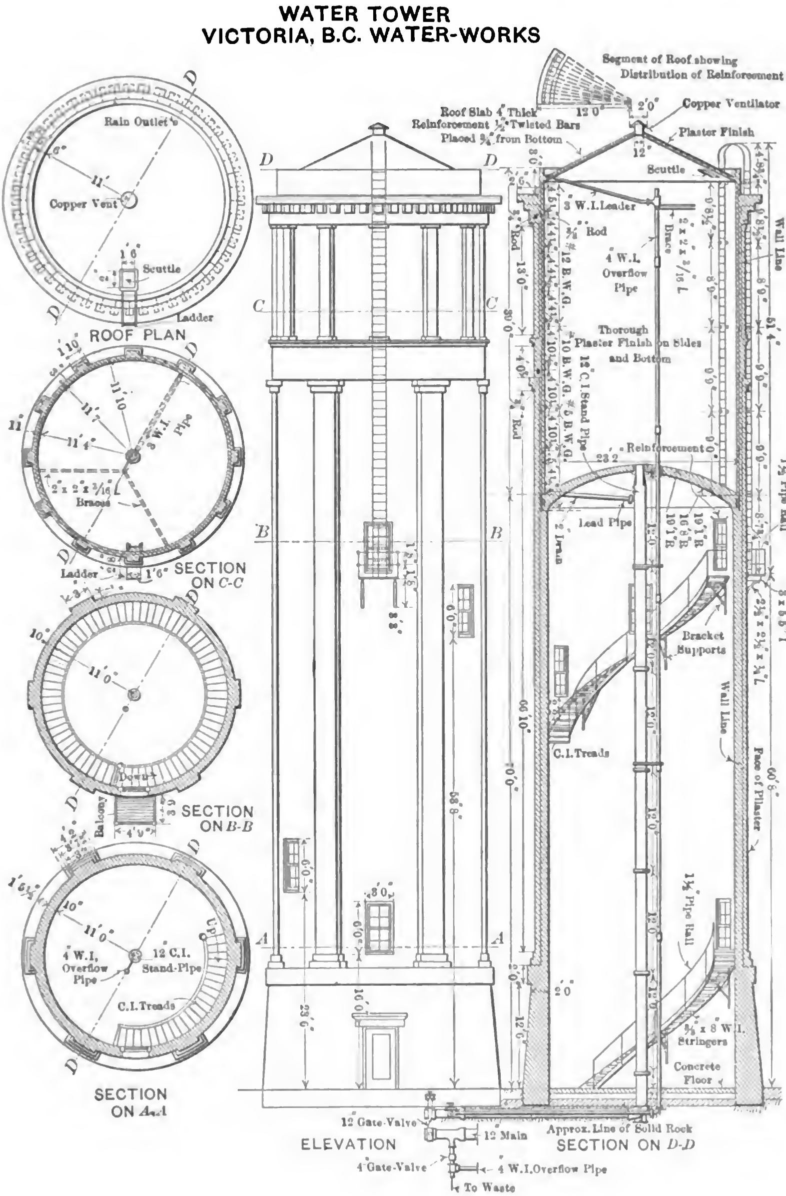
WATER TOWER
VICTORIA, B.C. WATER-WORKS
ROOF PLAN
SECTION ON C-C
SECTION ON B-B
SECTION ON A-A
ELEVATION
SECTION ON D-D
Segment of Roof showing Distribution of Reinforcement
Roof Slab 4" Thick
Reinforcement ½" Twisted Bars Placed ¾" from Bottom
Copper Ventilator
Plaster Finish
Scuttle
Copper Vent
Rain Outlet
Ladder
3 W.I. Leader
4 W.I. Overflow Pipe
Thorough Plaster Finish on Sides and Bottom
12 C.I. Stand Pipe
Reinforcement
Lead Pipe
Bracket Supports
Wall Line
Face of Plaster
C.I. Treads
1½ Pipe Rail
Concrete Floor
Approx. Line of Solid Rock
12 Gate-Valve
12 Main
4 Gate-Valve
4 W.I. Overflow Pipe
To Waste
Balcony
Braces
Stand-Pipe

FIG. 1.

was used, being made by double dressing 2 by 6-in. pieces to $1\frac{3}{4}$ by $5\frac{13}{16}$ in., and ripping on a diagonal, thus making two staves, 3 in. wide at one end and $2\frac{3}{4}$ in. wide at the other. This tapered lagging was used again on the 4-ft. belt and cornice forms, the taper being turned alternately up and down.

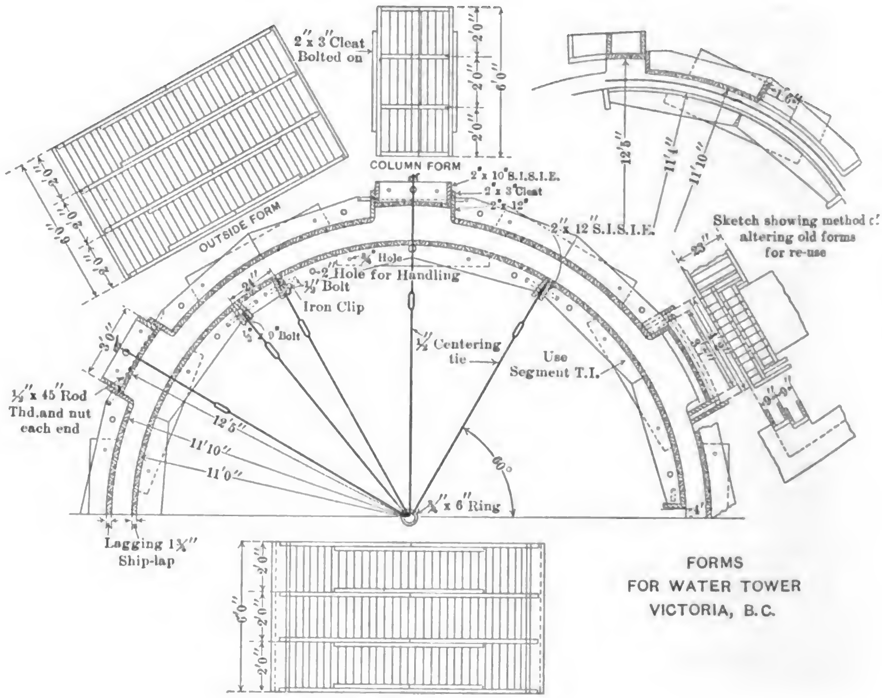

Fig. 2.

The interior diameter being uniform up to the bottom of the dome, collapsible forms were used from the beginning. These forms were constructed in six large sections, 6 ft. high, with one small key section with wedge piece to facilitate stripping, as shown in Fig. 2. There were three tiers of these, bolted end to end horizontally and to each other vertically.

Above the taper base and except in the 4-ft. belt and cornice, collapsible forms were used on the outside also. There were six sections extending from column to column and six column sections, all

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bolted together circumferentially and constructed as shown in Fig. 2. Three tiers of these were also bolted together both vertically and horizontally.

Having filled the top tier, the mode of operation was as follows:

All horizontal bolts in the lower inside and outside forms were removed, as was also the small key section on the inside; this left each section suspended to the corresponding one immediately above it by the vertical bolts before mentioned. It is thus seen that in each case the center tier performed the double duty of holding the upper tier, which was full of green concrete, and the sections of the lower tier, until they were hoisted up and again placed in position to be filled.

These lower forms were then hoisted by hand—four-part tackles being used—and placed in position on the top forms, their bottom edges being carefully set flush with the top edge of the form already in position, and then bolted to it. On the outside, the column forms, and on the inside, the wedge and key sections were set last. A 3-lb. plumb-bob on a fine line was suspended from the inner scaffold and carefully centered over a point set in the rock at the base. This line was in the exact center of the tower, and the tops of all the forms, after each shift, were carefully set from it by measurement, thus keeping the structure plumb.

The first 23 in. of the barrel of the tower was moulded with special outside forms, constructed so as to form the bases of the large pilasters. After eleven applications of the 6-ft. forms, these 23-in. sections were reversed to form the capitals, thus making these pilasters, 69 ft. 10 in. over all.

The forms of the 4-ft. belt and beading were made in twelve sections of simple segments and vertical lagging, as shown in Fig. 2.

Two sets of the outside forms were split longitudinally, as shown in Fig. 2, and used to form the small pilasters. The first set was put in place, filled, and the concrete allowed to harden. The bolts were loosened and the forms raised 5½ in. vertically, again bolted up, and the second set was placed in position, bringing the top of the second set up to the bottom of the cornice. The bases and capitals of the small pilasters were moulded on afterward.

The cornice forms are clearly shown in Fig. 2. The small boxes separating the dentils are made of light stuff, and tacked into the

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cornice forms so that, in stripping, they would remain in place and could be taken out separately, in order to prevent breaking off the corners of the dentils. A number of outside and inside sections were sawed in half horizontally in order to provide forms for the parapet wall.

The inside diameter of the tank is 8 in. greater than the inside diameter of the base. Two sets of inside forms were split longitudinally and opened out, as shown in Fig. 2, and another small section was added to complete the circle. The remaining set was left in place to support the dome forms.

The dome forms were made in twelve sections, bolted together to facilitate stripping. All ribs and segments were cut to size on the ground, put together in place, and then covered with lagging and two-ply tar paper. The lagging on the lower sharp curve was formed of a double thickness of $\frac{3}{8}$-in. spruce, the remainder being 1 by 4-in. pine, sized to a uniform thickness of $\frac{7}{8}$ in. Fig. 3 shows the construction of these forms and the method of putting on the lagging.

The roof forms were made in eight sections and bolted together to facilitate stripping. All ribs and segments were cut to size on the ground, put together in place, and covered with 1 by 4-in. lagging, dressed to a uniform thickness of $\frac{7}{8}$ in., and two-ply tar paper. Fig. 3 shows the construction of these forms. The segments being put in horizontally instead of square with the lagging, gave circles instead of parabolas, making them much easier to lay out, and giving a form which was amply stiff.

The question of using an inside scaffold only was carefully considered, but owing to the considerable amount of ornamentation on the outside, necessitating a large number of individual forms, it was not thought that any economy would result.

Fig. 4 and Figs. 1 and 2, Plate XXIII, show clearly the construction of the scaffolding.

All concrete was mixed wet, in a motor-driven, Smith mixer, and handled off the outside scaffold, being sent up in wheel-barrows on the ordinary contractor's hoist and placed in the forms through an iron chute having a hopper mouth. This chute was built in three sections bolted together, either one, two, or three sections being used, depending on the distance of the forms below the deck. When the top of the forms reached the elevation of any deck, the concrete was put

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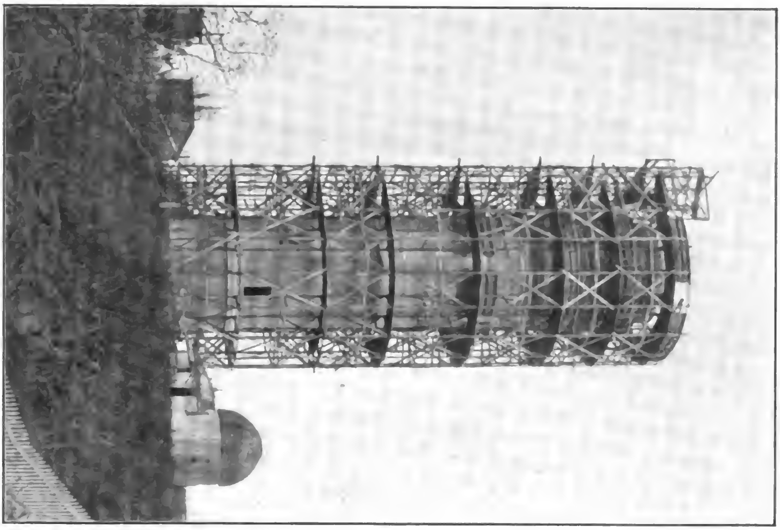

FIG. 1.—SCAFFOLDING FOR WATER TOWER.

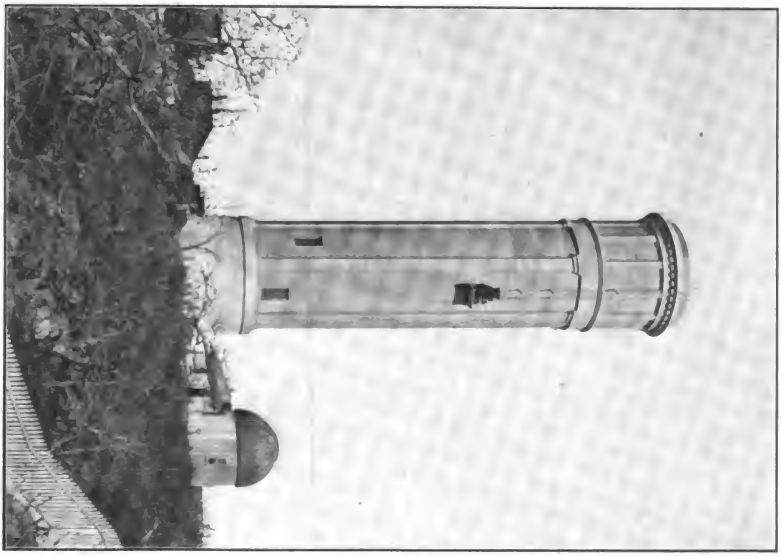

FIG. 2.—COMPLETED WATER TOWER.

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in through the chute from the deck above. The chute was light and easily shifted by the wheel-barrow men, assisted by the man placing the concrete, during the interval between successive wheel-barrows.

The concrete, except that for the roof and parapet, was composed of sand and broken rock, the run of the crusher being used. That for the roof and parapet was composed of sand and gravel. The only reason for using gravel for the concrete of the roof was the ease with

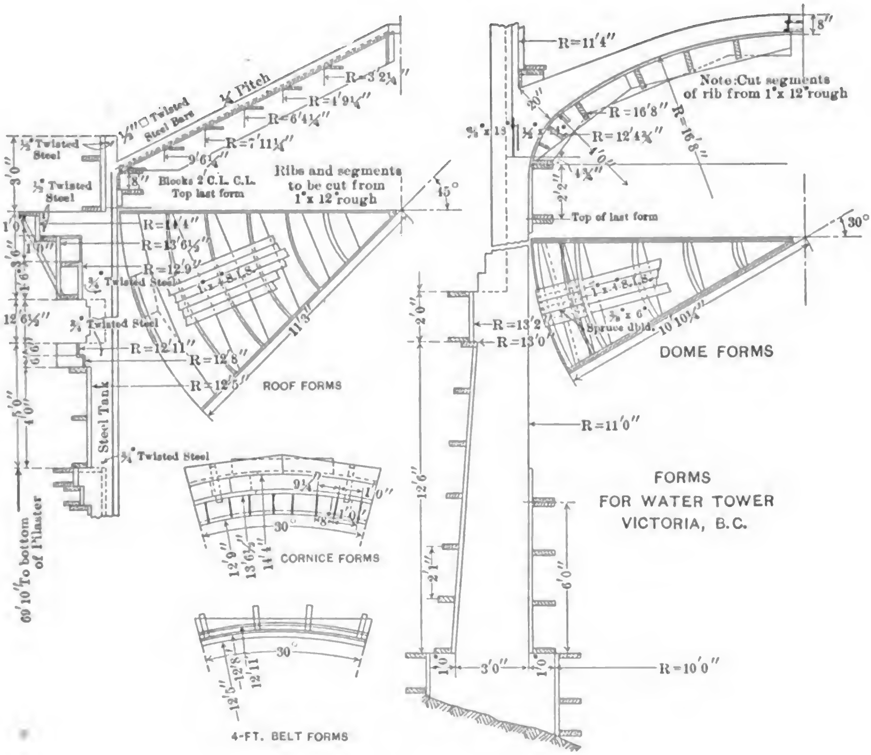

Fig. 8.

which it could be obtained in small quantities, the supply of broken rock having been used up, and this being the last concrete work to be done.

The concrete used was as follows: 1:3:6 for the sub-base and taper base; 1:3:5 for the barrel of the tower and tank casing; and 1:2:4 for the dome and roof. The dome was put in at one time, there being no joint, the same being true of the roof. Vancouver Portland cement,

manufactured on the island about 15 miles from the city, was used throughout the work.

Before filling, the inside of the tank was given a plaster coat, consisting of 1 part cement to $1\frac{3}{4}$ parts of fine sand. This proved to be insufficient to prevent leakage, the water seeping through the dome and appearing on the outside of the structure along the line of the bottom of the rings. Three more coats were then applied over the entire tank, and two additional ones over the dome and about 8 ft. up on the sides, and, except for one or two small spots which show just a sign of moisture, the tank is perfectly tight.

The barrel of the tower was carried up to a height of 66 ft. A special set of inside forms, about 2 ft. high, extending to the springing line of the dome, was then put in, and the dome forms were set up on it. The idea was that this 2-ft. form could be knocked out piece by piece and the weight of the dome form taken on wedges to the last 6-ft. form, these wedges being gradually slackened down in order to allow the dome form to settle clear of the dome. As a matter of fact, this was done, but the dome forms, being very tight, did not settle, and had to be pried off a section at a time. A similar method was used for slacking down the roof forms, with similar results.

After the dome forms had been put in, the concrete was carried up approximately to the elevation of the bottom of the rings. Small neat cement pads were then put in and accurately leveled, and on these the steel rings were placed, and the steel tank was erected.

In order to insure a perfectly round tank, each course was erected against wooden templates accurately centered and fastened to the inside scaffold. The tank is the ordinary type of light steel, the lower course being $\frac{3}{16}$-in., the next, No. 8 B. w. gauge, the next, No. 10 B. w. gauge, and the remaining four, No. 12 B. w. gauge.

Work on the foundation was started on August 15th, 1908, and the tower was not completed until April 1st, 1909. Much time was lost waiting for the delivery of the steel, and also owing to a period of very cold weather which caused entire cessation of work for about one month.

The tower as completed presents a striking appearance. In order to obliterate rings due to the successive application of the forms and to cover the efflorescence so common to concrete structures, the outside was given two coats of neat cement wash applied with ordinary calci-

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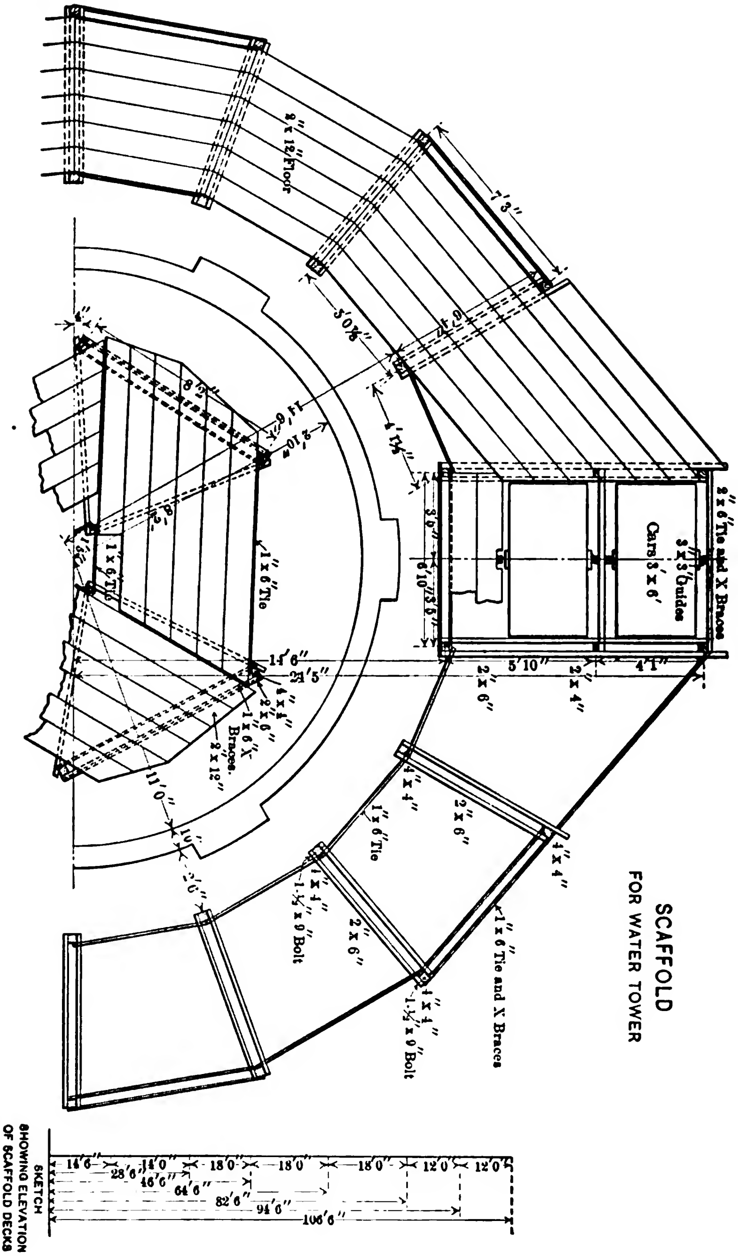
SCAFFOLD
FOR WATER TOWER
SKETCH
SHOWING ELEVATION
OF SCAFFOLD DECKS

FIG. 4.

mining brushes, and, up to the present time, this seems to have been very effective in accomplishing the desired result.

Irregularities due to forms are unnoticeable at a distance of 200 or 300 ft., and the grouting gave a very uniform color.

The application of two coats of cement wash cost, for labor, $97.68, and for material, $15.18, or $1.32 per 100 sq. ft., labor being at the rate of $2.25 per 8 hours and cement costing $2.53 per bbl. delivered on the work.

The tower was designed by Arthur L. Adams, M. Am. Soc. C. E., under whose direction the plans for all the work of remodeling the water-works system were prepared and executed. The forms, scaffolding, etc., were designed by the writer, who was also in immediate charge of the erection.

Tenders received for the construction of the tower covered an extremely wide range, and indicated at once the utter lack of knowledge on the part of the bidders of the cost of a structure of this kind. Inasmuch as none of them had had previous experience in this class of construction, the engineer deemed it the part of wisdom and economy to retain the construction under his immediate supervision, and, therefore, the work was done by days' labor.

Table 1 gives the cost of the structure. The total herein given will not coincide with the total cost as shown by the city's books, for the reason that various items not properly chargeable to the structure itself have been omitted, the principal ones of which are the cost of the site, the laying of about 600 ft. of sewer pipe to connect with the overflow, and considerable expense incident to the construction of a wagon road to the tower.

The rates of wages paid, all being on a basis of an 8-hour day, were as follows:

Common labor	$2.25 and $2.50
Carpenter	4.00
Carpenter's helper	2.75
Boiler-maker	3.50
Holders on	2.50
Boiler-maker foreman	5.00
Plasterers	6.00
Plasterers' helpers	3.00

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The cost of material was as follows:

Cement, per barrel....................	$2.53
Sand, per yard........................	1.47
Rock, per yard........................	0.80
Lumber, per 1 000 ft. b. m.............	14.00 and 16.00

All these prices are for material delivered on the work.

An examination of the cost data, as given, will show that for the most part the unit costs are very high. This is due chiefly to the continued interruption of the work, during its later stages, owing to bad weather, particularly in the case of the erection of the steel tank. The material cost in this case was also exceedingly high.

In the case of the concreting, inability to purchase a hoist and motor and the high cost of renting the same, together with the delays mentioned, added greatly to the unit cost.

When it is considered that the cost of plastering covers that of four coats over the entire inside of the tank and three more over about one-third of it, it does not appear so high, especially in view of the high rate of wages paid.

The cost per yard for concrete alone was $25.126, and this is probably about 25% in excess of the cost of the same class of work executed under more favorable conditions as to location, weather conditions, etc.

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TABLE 1.—COST OF HIGH-LEVEL TOWER, VICTORIA WATER-WORKS.
(412 cu. yd.)

	Total Cost.			Unit Cost.	
	Rate per hour.	Amount.	Complete.	Labor.	Material.
Preliminary Work:					
Labor, Carpenter	$0.50	$11.00			
Labor	0.344	64.94			
"	0.281	249.67	$325.61	$0.790	
Material		183.62	183.62		$0.324
Forms:					
Building, shifting, and stripping:					
Labor, Carpenter	0.50	1 832.99			
Labor	0.344	80.85			
"	0.281	563.84	2 477.68	6.014	
Material:					
Lumber		588.49			
Hardware		325.51			
Miscellaneous		18.90	932.90		2.240
Scaffold:					
Erecting and tearing down:					
Labor, Carpenter	0.50	693.00			
Labor	0.344	350.59			
"	0.281	117.27	1 160.86	2.818	
Material:					
Lumber		487.77			
Hardware		202.79	690.56		1.676
Concreting:					
Labor	0.50	142.00			
"	0.344	11.00			
"	0.281	947.81	1 100.81	2.672	
Material:					
Rock		317.30			
Sand		335.72			
Cement		1 591.97			
Motor and Hoist:					
Rental		406.56			
Power		83.58	2 735.08		6.638
Plastering (3 000 sq. ft.):					
Labor, Plasterers	0.75	116.50			
Labor	$0.46\frac{7}{8}$	15.00			
"	$0.37\frac{1}{2}$	198.52			
"	0.281	105.66	435.68	14.52 per sq. ft.	
Material:					
Sand		8.64			
Cement		66.10			
Alum and Potash		16.00	90.74	3.25 per sq. ft.	
Cement Wash (8 560 sq. ft.):					
Labor	$0.43\frac{3}{4}$	50.00			
"	0.281	47.68	97.68	1.14 per 100 sq. ft.	
Material:					
Cement		15.18	15.18	0.18 " " " "	
Windows, doors, and scuttle:					
Labor	0.50	49.00	49.00		
Material:					
1 door, 7 windows, etc.		47.26	47.26		
Equipment:					
40% of $461.46		184.58	184.58	0.448	
Superintendence			1 241.45	1.506	

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TABLE 1.—(*Continued.*)

	Total Cost.			Unit Cost.	
	Rate per hour.	Amount.	Complete.	Labor.	Material.
Steel Tank:					
Labor, Carpenter	$0.50	$124.24			
Helper	0.344	2.75			
Boiler-makers		382.57			
Holders on		147.33			
Labor		40.61			
Foreman	0.625	186.25	$883.75	$0.0441 per lb.	
Material:					
Tank, rivets, etc. (20 000 lb.)			1 740.69		$0.0875
Iron-work:					
Spiral stairway, inlet, and overflow pipes, ventilator, reinforcing steel, etc.:					
Labor, Machinists	0.50	89.50			
Helper	0.344	240.16			
Labor	0.281	100.79	430.45		
Material		1 814.71	1 814.71		
Total			$16 578.29		

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DISCUSSION

Mr. Couchot.

MAURICE C. COUCHOT, M. AM. SOC. C. E. (by letter).—It appears to the writer that in the design of this structure two features are open to criticism. The first is that such a high structure was built of plain concrete without any reinforcement. Even if the computation of stresses did not show the necessity for steel reinforcement, some should have been embedded in the work. As a matter of fact, the writer believes that, with the present knowledge of the benefit of reinforced concrete, a structure such as this should not be built without it. This applies mainly to the tower below the tank.

The second feature, which is still more important, refers to the insertion of a shell of smooth steel plate to take the stresses due to the hydrostatic pressure, and also to insure against leakage in the walls of the tank. The 6-in. shell of plain concrete outside the steel shell, and the 3-in. shell inside, do not work together, and are practically of no value as walls, but are simply outside and inside linings. Although the designer provided lugs to insure the adhesion of the concrete to the plate, such precaution, in the writer's opinion, will not prevent the separation of the concrete from the smooth steel plate, and, at some future time, the water will reach and corrode the steel. It would have been better to have reinforced the wall of the tank with rods, as is generally done. The full thickness would have been available, and less plastering would have been required. Furthermore, the adhesion of concrete to a smooth steel plate is of doubtful value, for, in reinforced concrete, it is not the adhesion which does the work, but the gripping of the steel by the concrete in the process of setting.

Mr. Mensch.

L. J. MENSCH, M. AM. SOC. C. E. (by letter).—This water-tower is probably the sightliest structure of its kind in North America; still, it does not look like a water-tower, and, from an architectural point of view, the crown portion is faulty, because it makes the tank appear to be much less in depth than it really is.

The cost of this structure far exceeds that of similar tanks in the United States. The stand-pipe at Attleboro, 50 ft. in diameter and 100 ft. high, cost about $25 000. Several years ago the writer proposed to build an elevated tank, 60 ft. in diameter and 40 ft. deep, the bottom of which was to be 50 ft. above the ground, for $21 000.

Among other elevated tanks known to the writer is one having a capacity of 100 000 gal., the bottom being 60 ft. above the ground.* The total quantities of material required for this tank are given as 4 480 cu. ft. of concrete, 23 200 lb. of reinforcing steel, and 27 600 ft., b. m., of form lumber and staging. Calculating at the abnormally

*"The Reinforced Concrete Pocket Book," p. 124.

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high unit prices of 40 cents per cu. ft. for concrete, 4 cents per lb. for steel, and $50 per 1 000 ft., b. m., for lumber, the cost of the concrete would be $1 792, the steel, $928, and the form lumber and staging, $1 380. Adding to this the cost of a spiral staircase, at the high figure of $7 per linear foot in height, the total cost of this structure would be $4 598. The factor of safety used in this structure was four, but some engineers who are not familiar with concrete construction may require a higher factor. By doubling the quantities of concrete and steel, which would mean a tensile stress in the steel of only 8 000 lb. per sq. in., and a compressive stress in the concrete of only 225 lb. per sq. in., the cost of the tank would be only $7 318, as compared with the $16 578 mentioned in the paper. This enormous discrepancy between a good design and an amateur design, and between day-labor work and contract work should be a lesson which consulting engineers and managers of large corporations, who prefer their own designs and day-labor work, should take to heart. Mr. Mensch.

A. H. Markwart, Assoc. M. Am. Soc. C. E. (by letter).—It is the writer's opinion that the steel tank enclosed within the concrete of the upper cylinder, to take up the hoop tension and presumably to provide a water-tight tower, will not fulfill this latter requirement. If a plastered surface on the dome-shaped bottom provided the necessary imperviousness, it would seem that plastered walls would have proved satisfactory. Mr. Markwart.

Apparently, the sheet-metal tank is intended to exclude the possibility of exterior leakage, but it occurs to the writer that it will fail to be efficient in this particular, because, under pressure, the water will force itself under the steel tank and the dome thrust rings and out to the exterior of the tower just below the tank, thus showing that insurance against leakage is actually provided by the plastered interior surfaces and not by the sheet-metal tank, and, for this reason, ordinary deformed rod reinforcement, in the writer's opinion, would have proved cheaper and better, and more in line with other parts of the reinforcement.

Mr. Kempkey states:

"Before filling, the inside of the tank was given a plaster coat, consisting of 1 part cement to 1¾ parts of fine sand. This proved to be insufficient to prevent leakage, the water seeping through the dome and appearing on the outside of the structure along the line of the bottom of the rings. Three more coats were then applied over the entire tank, and two additional ones over the dome and about 8 ft. up on the sides, and, except for one or two small spots which show just a sign of moisture, the tank is perfectly tight."

This substantiates the writer's contention that water-tightness was actually obtained by a liberal use of cement plaster, which would also have been true had the reinforcement been rods.

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Mr. Markwart. As a further comment, it might be stated that a water-tight concrete for the tank could have been obtained by adding from 8 to 10% of hydrated lime to the 1:2:4 mixture. This seems advisable in all cases where a water-tight concrete is necessary. The interior plastering could then have been done as a further precaution.

Mr. Kempkey. A. Kempkey, Jr., Jun. Am. Soc. C. E. (by letter).—Mr. Couchot's statement, that the 3-in. inside and outside sheets forming the tank casing do not act together, is quite true, and it was not expected that they would, other than to protect the steel and form an ornamental covering for it.

There is certainly adhesion between concrete and steel, even though the steel be in the form of a thin shell, and in a structure of this kind where the steel is designed, with a low unit stress, to take all the strain, and where the load is at all times quiescent, it is difficult to see how this bond can be destroyed; the writer feels no concern on this score.

Mr. Markwart's statement, that the steel tank enclosed within the concrete of the upper cylinder, presumably to provide a water-tight tower, will not fulfill this latter requirement, is not true, as shown by the statement in the paper that the only leakage which occurred was that which passed under the tank, the entire remaining portion being absolutely tight. The amount of leakage, while insignificant, was, until remedied, sufficient to spot the outside of the tower, making it unsightly; and this, in the writer's opinion, is just what would have happened had the tank been constructed in the ordinary manner, with deformed bars, except that it would have extended over more or less of the entire surface, instead of being localized, as was actually the case, and would have required more instead of less plastering. It is also doubtful whether the addition of hydrated lime would have produced a tight tank, in the sense that this structure was required to be tight.

In the paper the writer endeavored to bring out the fact that this is one of the few instances where the æsthetic design of a structure of this sort is of prime importance, and cost a secondary consideration. There is, therefore, no use in comparing its cost with that of a structure in no way its equal in this respect and the use of which would not have been permitted any more than the use of the ordinary type of steel structure, even though the estimated cost were 75% less.

Mr. Mensch has been pleased to term this design amateurish, presumably because of the conservative character of the stresses used and because of its cost; at the same time, he sets up the design to which he makes reference as a good one simply because of its cheapness. He will find the "enormous discrepancy," to which he calls attention, accounted for by the fact that the "good design" would not have been tolerated because of its appearance and because of the fact that

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the excessively high unit stresses, of which Mr. Mensch is an exponent, did not commend themselves either to the designer, in common with most engineers, or to Victorian taste; while the design used has proven eminently satisfactory to a more than usually conservative and discriminating community. Mr. Kempkey.

Mr. Mensch's statement of unit costs, even though applied to a much plainer structure, is not calculated to inspire confidence in the soundness of his deductions in any one familiar with Victoria conditions.

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AMERICAN SOCIETY OF CIVIL ENGINEERS
INSTITUTED 1852

TRANSACTIONS

Paper No. 1174

PRESSURE, RESISTANCE, AND STABILITY OF EARTH.*

By J. C. Meem, M. Am. Soc. C. E.

With Discussion by Messrs. T. Kennard Thomson, Charles E. Gregory, Francis W. Perry, E. P. Goodrich, Francis L. Pruyn, Frank H. Carter, and J. C. Meem.

In the final discussion of the writer's paper, "The Bracing of Trenches and Tunnels, With Practical Formulas for Earth Pressures,"† certain minor experiments were noted in connection with the arching properties of sand. In the present paper it is proposed to take up again the question of earth pressures, but in more detail, and to note some further experiments and deductions therefrom, and also to consider the resistance and stability of earth as applied to piling and foundations, and the pressure on and buoyancy of subaqueous structures in soft ground.

In order to make this paper complete in itself, it will be necessary, in some instances, to include in substance some of the matter of the former paper, and indulgence is asked from those readers who may note this fact.

Experiment No. 1.—As the sand-box experiments described in the former paper were on a small scale, exception might be taken to them, and therefore the writer has made this experiment on a scale sufficiently large to be much more conclusive. As shown in Fig. 1, wooden

* Presented at the meeting of May 18th, 1910.
† *Transactions*, Am. Soc. C. E., Vol. LX, p. 1.

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abutments, 3 ft. wide, 3 ft. apart, and about 1 ft. high, were built and filled solidly with sand. Wooden walls, 3 ft. apart and 4 ft. high, were then built crossing the abutments, and solidly cleated and braced frames were placed across their ends about 2 ft. back of each abutment. A false bottom, made to slide freely up and down between the abutments, and projecting slightly beyond the walls on each side, was then blocked up snugly to the bottom edges of the sides, thus obtaining a box 3 by 4 by 7 ft., the last dimension not being important. Bolts, 44 in. long, with long threads, were run up through the false bottom and through 6 by 15 by 2-in. pine washers to nuts on the top. The box was filled with ordinary coarse sand from the trench, the sand

SECTIONS OF BOX-FRAME FOR SAND-ARCH EXPERIMENT

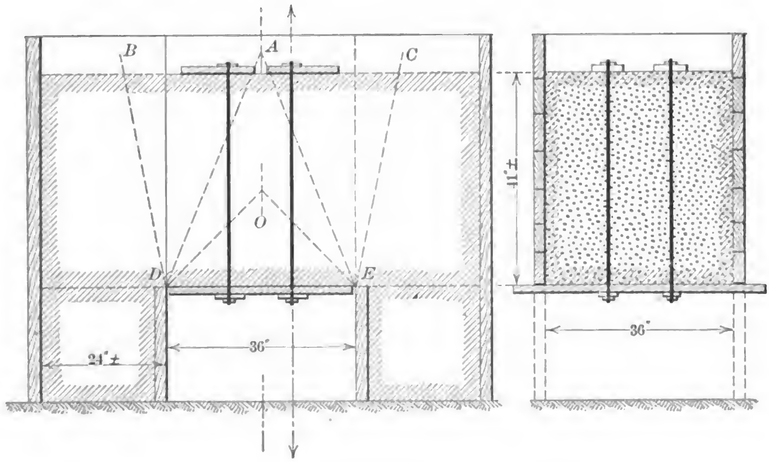

FIG. 1.

being compacted as thoroughly as possible. The ends were tightened down on the washers, which in turn bore on the compacted sand. The blocking was then knocked out from under the false bottom, and the following was noted:

As soon as the blocking was removed the bottom settled nearly 2 in., as noted in Fig. 1, Plate XXIV, due to the initial compacting of the sand under the arching stresses. A measurement was taken from the bottom of the washers to the top of the false bottom, and it was noted as 41 in. (Fig. 1). After some three or four hours, as the arch had not been broken, it was decided to test it under greater loading, and four men were placed on it, four others standing on the

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haunches, as shown in Fig. 2, Plate XXIV. Under this additional loading of about 600 lb. the bottom settled 2 in. more, or nearly 4 in. in all, due to the further compression of the sand arch. About an hour after the superimposed load had been removed, the writer jostled the box with his foot sufficiently to dislodge some of the exposed sand, when the arch at once collapsed and the bottom fell to the ground.

Referring to Fig. 2, if, instead of being ordinary sand, the block comprised within the area, *A U J V X,* had been frozen sand, there can be no reason to suppose that it would not have sustained itself, forming a perfect arch, with all material removed below the line, *V E J,* in fact, the freezing process of tunneling in soft ground is based on this well-known principle.

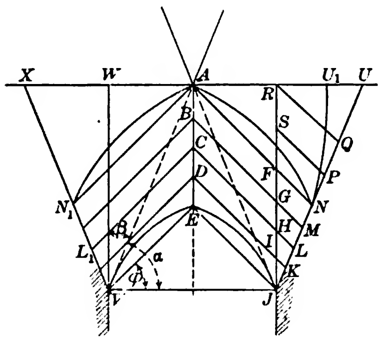

Fig. 2.

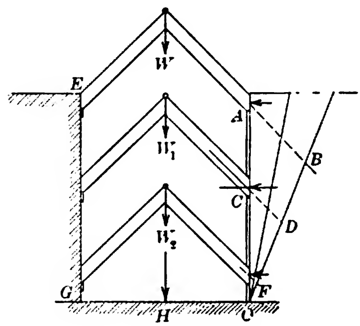

Fig. 3.

If, then, instead of removing the mass, *J E V,* it is allowed to remain and is supported from the mass above, one must concede to this mass in its normal state the same arching properties it would have had if frozen, excepting, of course, that a greater thickness of key should be allowed, to offset a greater tendency to compression in moist and dry as against frozen sand, where both are measured in a confined area.

If, in Fig. 2, $E\ V\ J = \phi =$ the angle of repose, and it be assumed that *A J,* the line bisecting the angle between that of repose and the perpendicular, measures at its intersection with the middle vertical (*A,* Fig. 2) the height which is necessary to give a sufficient thickness of key, it may be concluded that this sand arch will be self-sustaining. That is, it is assumed that the arching effect is taken up virtually within the limits of the area, $A\ N_1\ V\ E\ J\ N\ A$, thus relieving the

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FIG. 1.—INITIAL SETTLEMENT IN 3-FT. SAND ARCH, DUE TO COMPRESSION OF MATERIAL ON REMOVING SUPPORTS FROM BOTTOM.

FIG. 2.—FINAL SETTLEMENT OF SAND ARCH, DUE TO COMPRESSION IN EXCESS LOADING.

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structure below of the stresses due to the weight or thrust of any of the material above; and that the portion of the material below $V\ E\ J$ is probably dead weight on any structure underneath, and when sustained from below forms a natural "centering" for the natural arch above. It is also probably true that the material in the areas, $X\ N_1\ A$ and $A\ N\ U$, does not add to the arching strength, more especially in those materials where cohesion may not be counted on as a factor. This is borne out by the fact that, in the experiment noted, a well-defined crack developed on the surface of the sand at about the point U_1, and extended apparently a considerable depth, assumed to be at N, where the haunch line is intersected by the slope line from A.

In this experiment the sand was good and sharp, containing some gravel, and was taken directly from the adjoining excavation. When thrown loosely in a heap, it assumed an angle of repose of about 45 degrees. It should be noted that this material when tested was not compacted as much, nor did it possess the same cohesion, as sand in its normal undisturbed condition in a bank, and for this reason it is believed that the depth of key given here is absolutely safe for all except extraordinary conditions, such as non-homogeneous material and others which may require special consideration.

Referring again to the area, $A\ N_1\ V\ J\ N\ A$, Fig. 2, it is probable that, while self-sustaining, some at least of the lower portion must derive its initial support from the "centering" below, and the writer has made the arbitrary assumption that the lower half of it is carried by the structure while the upper half is entirely independent of it, and, in making this assumption, he believes he is adding a factor of safety thereto. The area, then, which is assumed to be carried by an underground structure the depth of which is sufficient to allow the lines, $V\ A$ and $J\ A$, to intersect below the surface, is the lower half of $A\ N_1\ V\ E\ J\ N\ A$, or its equivalent, $A\ V\ E\ J\ A$, plus the area, $V\ E\ J$, or $A\ V J\ A$, the angle, $A\ V\ J$, being $\alpha = \frac{1}{2}(90° - \phi) + \phi = 45° + \frac{\phi}{2}$.

It is not probable that these lines of thrust or pressure transmission, $A\ N$, $D\ K$, etc., will be straight, but, for purposes of calculation, they will be assumed to be so; also, that they will act along and parallel to the lines of repose of their natural slope, and that the thrust of the earth will therefore be measured by the relation between the radius and the tangent of this angle multiplied by the weight of material

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affected. The dead weight on a plane, $V\ J$, due to the material above, is, therefore, where

l = span or extreme width of opening = $V\ J$,

W = weight per cubic foot of material, and

W_1 = weight per linear foot.

$$W_1 = \frac{2 \times \frac{l}{2} \tan. \alpha \times W}{2} = \frac{1}{2}\, l \tan. \left\{ \frac{1}{2}\,(90° - \phi) + \phi \right\} W$$

$$= \frac{l}{2} \tan. \left(45° + \frac{\phi}{2}\right) W.$$

The application of the above to flat-arched or circular tunnels is very simple, except that the question of side thrust should be considered also as a factor. The thrust against the side of a tunnel in dry sand having a flat angle of repose will necessarily be greater than in very moist sand or clay, which stands at a much steeper angle, and, for the same reason, the arch thrust is greater in dryer sand and therefore the load on a tunnel structure should not be as great, the material being compact and excluding cohesion as a factor. This can be illustrated by referring to Fig. 3 in which it is seen that the flatter the position of the "rakers" keying at W_1, W_2, and W, the greater will be the side thrust at A, C, and F. It can also be illustrated by assuming that the arching material is composed of cubes of polished marble set one vertically above the other in close columns. There would then be absolutely no side thrust, but, likewise, no arching properties would be developed, and an indefinite height would probably be reached above the tunnel roof before friction enough would be developed to cause it to relieve the structure of any part of its load. Conversely, if it be assumed that the superadjacent material is composed of large bowling balls, interlocking with some degree of regularity, it can be seen that those above will form themselves into an arch over the "centering" made up of those supported directly by the roof of the structure, thus relieving the structure of any load except that due to this "centering."

If, now, the line, $A\ B$, in Fig. 4, be drawn so as to form with $A\ C$ the angle, β, to be noted later, and it be assumed that it measures the area of pressure against $A\ C$, and if the line, $C\ F$, be drawn, forming with $C\ G$, the angle, α, noted above, then $G\ F$ can be reduced in some measure by reason of the increase of $G\ C$ to $C\ B$, because the side thrust above the line, $B\ C$, has slightly diminished the loading above. The writer makes the arbitrary assumption that this decrease in $G\ F$

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should equal 20% of $B\ C = F\ D_1$. If, then, the line, $B\ D_1$ be drawn, it is conceded that all the material within the area, $A\ B\ D_1\ G\ C\ A$, causes direct pressure against or upon the structure, $G\ C\ A$, the vertical lines being the ordinates of pressure due to weight, and the horizontal lines (qualified by certain ratios) being the abscissas of pressure due to thrust. An extreme measurement of this area of pressure is doubtless approximately more nearly a curve than the straight lines given, and the curve, $A\ R\ T\ I\ D_{II}$, is therefore drawn in to give graphically and approximately the safe area of which any

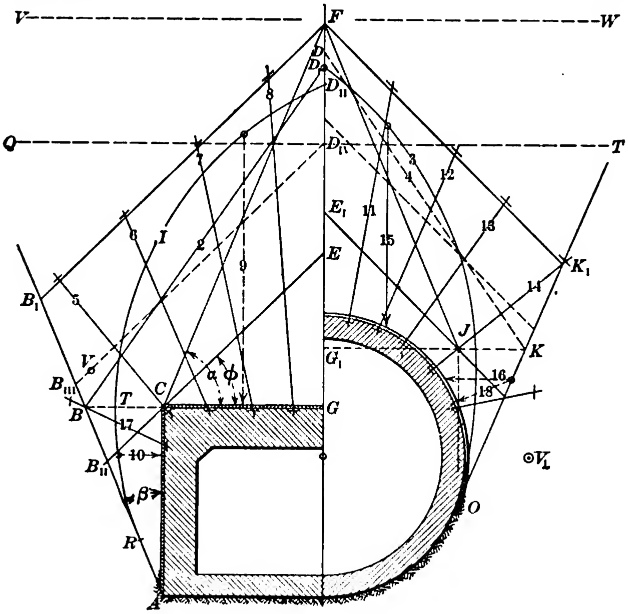

Fig. 4.

vertical ordinate, multiplied by the weight, gives the pressure on the roof at that point, and any horizontal line, or abscissa, divided by the tangent of the angle of repose and multiplied by the weight per foot, gives the pressure on the side at that point.

The practical conclusion of this whole assumption is that the material in the area, $F\ E\ C\ B\ B_1$, forms with the equivalent opposite area an arch reacting against the face, $C\ B\ B_1$, and that, as heretofore noted, the lower half (or its equivalent, $B\ D_1\ G\ B$) of the weight of this is assumed to be carried by the structure, the upper half being self-sustaining, as shown by the line, $B_{III}\ D_{IV}$ (or, for absolute safety,

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the curved line), and therefore, if rods could be run from sheeting inside the tunnel area to a point outside the line, $F\ B_1$, as indicated by the lines, 5, 6, 7, 8, 11, 12, 13, etc., that the internal bracing of this tunnel could be omitted, or that the tunnel itself would be relieved of all loading, whereas these rods would be carrying some large portion at least of the weight within the area circumscribed by the curve, $D_{II}\ I\ T\ G$, and further, that a tunnel structure of the approximate dimensions shown would carry its maximum load with the surface of the ground between D_{IV} and F, beyond which point the pressure would remain the same for all depths.

In calculating pressures on circular arches, the arched area should first be graphically resolved into a rectangular equivalent, as in the right half of Fig. 4, proceeding subsequently as noted.

The following instances are given as partial evidence that in ordinary ground, not submerged, the pressures do not exceed in any instance those found by the above methods, and it is very probable that similar instances or experiences have been met by every engineer engaged in soft-ground tunneling:

In building the Bay Ridge tunnel sewer, in 62d and 64th Streets, Brooklyn, the arch timber bracing shown in Fig. 1, Plate XXVI, was used for more than 4 000 ft., or for two-thirds of the whole 5 800 ft. called for in the contract. The external width of opening, measured at the wall-plate, averaged about 19 ft. for the 14½-ft. circular sewer and 19½ ft. for the 15-ft. sewer. The arch timber segments in the cross-section were 10 by 12-in. North Carolina pine of good grade, with 2 in. off the butt for a bearing to take up the thrust. They were set 5 ft. apart on centers, and rested on 6 by 12-in. wall-plates of the same material as noted above. The ultimate strength of this material, across the grain, when dry and in good condition, as given by the United States Forestry Department tests is about 1 000 lb. in compression. Some tests* made in 1907 by Mr. E. F. Sherman for the Charles River Dam in Boston, Mass., show that in yellow pine, which had been water-soaked for two years, checks began to open at from 388 to 581 lb. per sq. in., and that yields of ¼ in. were noted at from 600 to 1 000 lb. As the tunnel wall-plates described in this paper were subject to occasional saturation, and always to a moist atmosphere, they could never have been considered as equal to dry material. Had

* *Engineering News*, July 1st, 1909.

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FIG. 1.--NORMAL SLOPES AND STRATA OF NEWLY EXCAVATED BANKS.

FIG. 2.—NORMAL SLOPES AND STRATA OF NEWLY EXCAVATED BANKS.

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the full loading shown by the foregoing come on these wall-plates, they would have been subjected to a stress of about 25 tons each, or nearly one-half of their ultimate strength. In only one or two instances, covering stretches of 100 ft. in one case and 200 ft. in another, where there were large areas of quicksand sufficient to cause semi-aqueous pressure, or pockets of the same material causing eccentric loading, did these wall-plates show any signs of heavy pressure, and in many instances they were in such good condition that they could be taken out and used a second and a third time. Two especially interesting instances came under the writer's observation: In one case, due to a collapse of the internal bracing, the load of an entire section, 25 ft. long and 19 ft. wide, was carried for several hours on ribs spaced 5 ft. apart. The minimum cross-section of these ribs was 73 sq. in., and they were under a stress, as noted above, of 50 000 lb., or nearly up to the actual limit of strength of the wall-plate where the rib bore on it. When these wall-plates were examined, after replacing the internal bracing, they did not appear to have been under any unusual stress.

In another instance, for a distance of more than 700 ft., the sub-grade of the sewer was 4 ft. below the level of the water in sharp sand. In excavating for "bottoms" the water had to be pumped at the rate of more than 300 gal. per min., and it was necessary to close-sheet a trench between the wall-plates in which to place a section of "bottom." In spite of the utmost care, some ground was necessarily lost, and this was shown by the slight subsidence of the wall-plates and a loosening up of the wedges in the supports bearing on the arch timbers. During this operation of "bottoming," two men on each side were constantly employed in tightening up wedges and shims above the arch timbers. It is impossible to explain the fact that these timbers slackened (without proportionate roof settlement) by any other theory than that the arching was so nearly perfect that it relieved the bracing of a large part of the load, the ordinary loose material being held in place by the arching or wedging together of the 2-in. by 3-ft. sheeting boards in the roof, arranged in the form of a segmental arch. The material above this roof was coarse, sharp sand, through which it had been difficult to tunnel without losing ground, and it had admitted water freely after each rain until the drainage of a neighboring pond had been completed, the men never being willing to resume work until the influx of water had stopped.

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The foregoing applies only to material ordinarily found under ground not subaqueous, or which cannot be classed as aqueous or semi-aqueous material. These conditions will be noted later.

The writer will take up next the question of pressures against the faces of sheeted trenches or retaining walls, in material of the same character as noted above. Referring to Fig. 2, it is not reasonable to suppose that having passed the line, *R F J,* the character of the stresses due to the thrust of the material will change, if bracing should be substituted for the material in the area, *W V J R,* or if, as in Fig. 3, canvas is rolled down along the lines, *E G* and *A O,* and if, as this section is excavated between the canvas faces, temporary struts are erected, there is no reason to believe that with properly

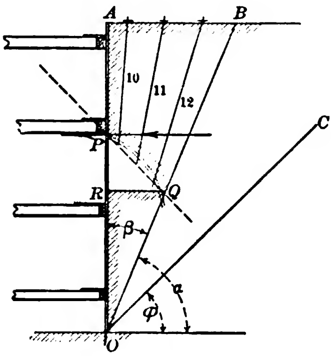

Fig. 5.

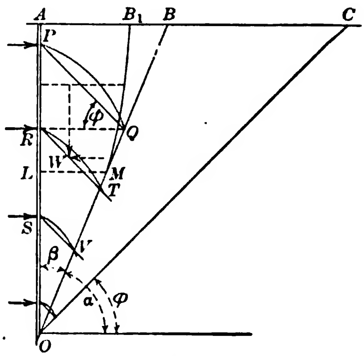

Fig. 6.

adjusted weights at *W* or W_2, an exact equilibrium of forces and conditions cannot be obtained. Or, again, if, as in Fig. 5, the face, *P Q,* is sheeted and rodded back to the surface, keying the rods taut, there is undoubtedly a stable condition and one which could not fail in theory or practice, nor can anyone, looking at Fig. 5, doubt that the top timbers are stressed more heavily than those at the bottom. The assumption is that the tendency of the material to slide toward the toe causes it to wedge itself between the face of the sheeting on the one hand and some plane between the sheeting and the plane of repose on the other, and that the resistance to this tendency will cause an arching thrust to be developed along or parallel to the lines, *A N, B M,* etc., Fig. 2, which are assumed to be the lines of repose, or curves approximating thereto. As the thrust is greatest in that material directly at the face, *A O,* Fig. 6, and is nothing at the plane

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of repose, $C\ O$, it may be assumed arbitrarily that the line, $B\ O$, bisecting this angle divides this area into two, in one of which the weight resolves itself wholly into thrust, the other being an area of no thrust, or wholly of weight bearing on the plane of repose. Calling this line, $B\ O$, the haunch line, the thrust in the area, $A\ O\ B$, is measured by its weight divided by the tangent of the angle, $P\ Q\ R = \phi$, which is the angle of repose; that is, the thrust at any given point, $R = R\ Q \div \tan.\ \phi$.

The writer suggests that, in those materials which have steeper angles of repose than 45°, the area of pressure may be calculated as above, the thrust being computed, however, as for an angle of 45 degrees.

In calculating the bending moment against a wall or bracing, there is the weight of the mass multiplied by the distance of its center of gravity vertically above the toe, or, approximately:

$$\text{Area, } A\ O\ B, \times \text{ weight per unit} \times \frac{2}{3} \text{ height,}$$

where h = height,

W = weight per cubic foot of material = 90 lb., and $\beta = \dfrac{90° - \phi}{2}$

P = pressure per linear foot (vertically),

$$\text{then } P = h \times \frac{h}{2} (\tan.\ \beta) \times W \times \frac{2}{3} h = \frac{1}{3} h^3\ W \tan.\ \beta.$$

When the angle of repose, ϕ, is less than 45°, this result must be reduced by dividing by tan. ϕ; that is, $h = \frac{1}{3} h^3 \tan.\ \beta \div \tan.\ \phi$.

Figs. 1 and 2, Plate XXV, show recently excavated banks of gravel and sand, which, standing at a general angle of 45°, were in process of "working," that is, there was continual slipping down of particles of the sand, and it may be well to note that in time, under exposure to weather conditions, these banks would finally assume a slope of about 33 degrees. They are typical, however, as showing the normal slope of freshly excavated sandy material, and a slope which may be used in ordinary calculations. The steps seen in Plate XXV show the different characteristics of ground in close proximity. In Fig. 2, Plate XXVI,* may be seen a typical bank of gravel and sand; it shows the well-defined slope of sand adjacent to and in connection with the cohesive properties of gravel.

The next points to be considered are the more difficult problems concerning subaqueous or saturated earths. The writer has made

*From "Gravel for Good Roads."

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some experiments which appear to be conclusive, showing that, except in pure quicksand or wholly aqueous material, as described later, the earth and water pressures act independently of each other.

For a better understanding of the scope and purpose of this paper, the writer divides supersaturated or subaqueous materials into three classes:

Class A.—Firm materials, such as coarse and fine gravels, gravel and sands mixed, coarse sands, and fine sands in which there is not a large proportion of fine material, such as loam, clay, or pure quicksand.

Class B.—Semi-aqueous materials, such as fine sands in which there is a large proportion of clay, etc., pure clays, silts, peats, etc.

Class C.—Aqueous materials, such as pure quicksands, in which the solid matter is so finely divided that it is amorphous and virtually held in suspension, oils, quicksilver, etc.

Here it may be stated that the term, "quicksand," is so illusive that a true definition of it is badly needed. Many engineers call quicksand any sand which flows under the influence of water in motion. The writer believes the term should be applied only to material so "soupy" that its properties are practically the same as water under static conditions, it being understood that any material may be unstable under the influence of water at sufficiently high velocities, and that it is with a static condition, or one approximately so, that this paper deals.

A clear understanding of the firm materials noted in Class A will lead to a better solution of problems dealing with those under Class B, as it is to this Class A that the experiments largely relate.

The experiments noted below were made with varying material, though the principal type used was a fine sand, under the conditions in which it is ordinarily found in excavations, with less than 40% voids and less than 10% of very fine material.

Experiment No. 2.—The first of these experiments, which in this series will be called No. 2, was simple, and was made in order to show that this material does not flow readily under ordinary conditions, when not coupled with the discharge of water under high velocity. A bucket 12 in. in diameter, containing another bucket 9 in. in diameter, was used. A 6 by 6-in. hole was cut in the bottom of the inner bucket. About 3 in. of sand was first placed in the bottom of the larger bucket and it was partly filled with water. The inside

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FIG. 1.—TYPES OF ARCH TIMBERS USED IN BAY RIDGE TUNNEL SEWER.

FIG. 2.—NORMAL SLOPE OF LOOSE SAND, GRAVEL, AND CEMENTED GRAVEL, IN CLOSE PROXIMITY.

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bucket was then given a false bottom and partly filled with wet sand, resting on the sand in the larger bucket. Both were filled with water, and the weight, *W*, Fig. 7, on the arm was shifted until it balanced the weight of the inside bucket in the water, the distance of the weight, *W*, from the pivot being noted. The false bottom was then removed and the inside bucket, resting on the sand in the larger one, was partly filled with sand and both were filled with water, the conditions at the point of weighing being exactly the same, except that the false bottom was removed, leaving the sand in contact through the 6 by 6-in. opening. It is readily seen that, if the sand had possessed the aqueous properties sometimes attributed to sand under water, that in the inside bucket would have flowed out through the square hole in the bottom, allowing it to be lifted by any weight in excess of the actual weight of the bucket, less its buoyancy, as would be the case if

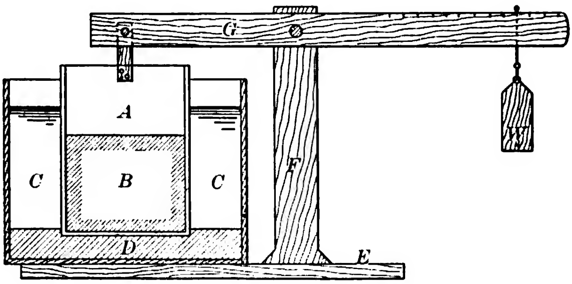

FIG. 7.

it contained only water instead of sand and water. It was found, however, that the weight, resting at a distance of more than nine-tenths of the original distance from the pivot, would not raise the inside bucket. On lifting this inside bucket bodily, however, the water at once forced the sand out through the bottom, leaving a hole almost exactly the shape and size of the bottom orifice, as shown in Fig. 1, Plate XXVII. It should be stated that, in each case, the sand was put in in small handfuls and thoroughly mixed with water, but not packed, and allowed to stand for some time before the experiments were tried, to insure the compactness of ordinary conditions. It is seen from Fig. 1, Plate XXVII, that the sand was stable enough to allow the bucket to be put on its side for the moment of being photographed, although it had been pulled out of the water a little less than 3 min.

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Experiment No. 3.—In order to show that the arching properties of sand are not destroyed under subaqueous conditions, a small sand-box, having a capacity of about 1 cu. ft., and similar to that described in Experiment No. 1, was made. The bottom was cut out, with the exception of a ¾-in. projection on two sides, and a false bottom was placed below and outside of the original bottom, with bolts running through it, keying to washers on top of the sand, with which the box was partly filled. One side of the box contained a glass front, in order that conditions of saturation could be observed. The box of sand was then filled with water and, after saturation had been completed and the nuts and washers had been tightened down, the box was lifted off the floor. There was found to be no tendency whatever for the bottom to fall away, showing conclusively that the arching properties had not been destroyed by the saturation of the sand.

The next three experiments were intended to show the relative pressure over any given area in contact with the water in the one case or sand and water in the other.

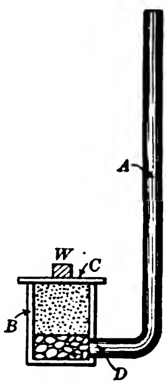

FIG. 8.

Experiment No. 4.—The apparatus for this experiment consisted of a 3-in. pipe about 4-in. long and connected with a ¾-in. goose-neck pipe 17 in. high above the top of the bowl shown in Fig. 8 and in Fig. 2, Plate XXVII. A loose rubber valve was intended to be seated on the upper face of the machined edge of the bowl and weighted down sufficiently to balance it against a head of water corresponding to the 17-in. head in the goose-neck. The bowl was then to be filled with sand and the difference, if any, noted between the weight required to hold the flap-valve down under the same head of water flowing through the sand. The results of this experiment were not conclusive, owing to the difficulty of making contact over the whole area of the sand and the rim of the bowl at the same time. At times, for instance, less than 1 lb. would hold back the water indefinitely, while, again, 2 or 3 lb. would be required as opposed to the 4½ lb. approximate pressure required to hold down the clear water. Again, at times the water would not flow through the neck at all, even after several hours, and after increasing the head by attaching a longer rubber tube

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FIG. 1.—EXPERIMENT SHOWING PROPERTIES OF SAND.

FIG. 2.—SAND PUSHED UP FROM BOWL BY WATER PRESSURE THROUGH GOOSE-NECK.

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thereto. In view of these conditions, this experiment would not be noted here, except that it unexpectedly developed one interesting fact. In order to insure against a stoppage of water, as above referred to, gravel was first put into the bottom of the bowl and the flap-valve was then rubbed down and held tightly while the pipe was filled. On being released, the pressure of water invariably forced out the whole body of sand, as shown in Fig. 2, Plate XXVII. Care was taken to see that the sand was saturated in each case, and the experiment was repeated numberless times, and invariably with the same result. The sand contained about 40% of voids. The deduction from this experiment is that the pressure of water is against rather than through sand and that any excess of voids occurring adjacent to a face against which there is pressure of water will be filled with sand, excepting in so far, of course, as the normal existing voids allow the pressure of the water to be transmitted through them.

If, then, the covering of sand over a structure is sufficiently heavy to allow arching action to be set up, the structure against which the pressure is applied must be relieved of much of the pressure of water against the area of sand not constituted as voids acting outside of the arching area. This is confirmed by the two following experiments:

Experiment No. 5.—The same apparatus was used here as in Experiment No. 2, Fig. 7, except that the inside bucket had a solid bottom. The inside and outside buckets were filled with water and the point was noted at which the weight would balance the inside bucket at a point some 3 in. off the bottom of the outside bucket. This point was measured, and the bottom of the larger bucket was covered over with sand so that in setting solidly in the sand the inside bucket would occupy the same relative position as it did in the water. The same weight was then applied and would not begin to lift the inner bucket. For instance, in the first part of the experiment the weight stood at 12 in. from the pivot, while in the next step the weight, standing at the end of the bar, had no effect, and considerable external pressure had to be exerted before the bucket could be lifted. Immediately after it was relieved, however, the weight at 12 in. would hold it clear of the sand. No attempt was made to work the bucket into the sand; the sand was leveled up and the bucket was seated on it, turned once or twice to insure contact, and then allowed to stand for some time before making the experiment. No attempt was made to establish the rela-

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tionship between sands of varying voids, the general fact only being established, by a sufficient number of experiments, that the weight required to lift the bucket was more than double in sand having 40% of voids than that required to lift the bucket in water only.

Experiment No. 6.—The apparatus for this experiment consisted essentially of a hydraulic chamber about 8 in. in diameter and 1 ft. high, the top being removable and containing a collar with suitable packing, through which a 2½-in. piston moved freely up and down, the whole being similar to the cylinder and piston of a large hydraulic jack, as shown in Fig. 1, Plate XXVIII. Just below the collar and

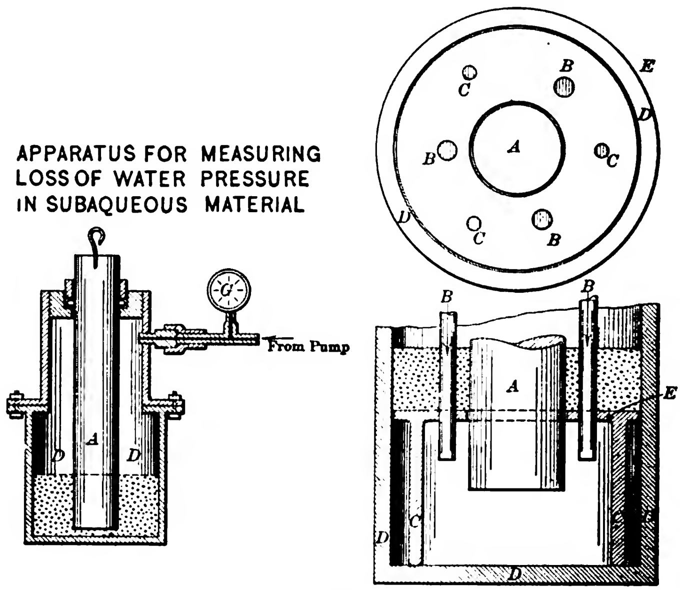

FIG. 9.

above the chamber there was a ½-in. inlet leading to a copper pipe and thence to a high-pressure pump. Attached to this there was a gauge to show the pressure obtained in the chamber, all as shown in Fig. 9. The purpose of the apparatus was to test the difference in pressure on any object submerged in clear water and on the same object buried in the sand under water. It is readily seen that, if pressure be applied to the water in this chamber, the amount of pressure (as measured by the gauge) necessary to lift the piston will be that due to the weight of the piston, less its displacement, plus the friction of the piston in the collar.

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FIG. 1.—APPARATUS FOR MEASURING LOSS OF PRESSURE IN SUBAQUEOUS MATERIALS.

FIG. 2.—RAISING ROOF OF BATTERY TUBES, IN BROOKLYN, BY "BLEEDING" SAND THROUGH DISPLACED PLATES.

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Now, if for any reason the bottom area of the piston against which the water pressure acts be reduced, it will necessarily require a proportionate amount of increase in the pressure to lift this piston. If, therefore, it is found that 10 lb., for illustration, be required to lift the piston when plunged in clear water, and 20 lb. be required to lift it when buried in sand, it can be assumed at once that the area of the piston has been reduced 50% by being buried in the sand, eliminating the question of the friction of the sand itself around the piston. In order to determine what this friction might be, the writer arranged a table standing on legs above the bottom of the chamber, allowing the piston to move freely through a hole in its center. Through this table pipes were entered (as shown in part of Fig. 9). The whole was then placed in the chamber with the piston in place, and the area above was filled with sand and water. It is thus seen that, the end of the piston being free and in clear water, the difference, if any, between the pressure required to lift the piston when in clear water alone and in the case thus noted, where it was surrounded by sand, would measure the friction of the sand on the piston. After several trials of this, however, it was clearly seen that the friction was too slight to be noted accurately by a gauge registering single pounds, that is, with a piston in contact with 6 in. of sand vertically, a friction of 25 lb. per sq. ft. would only require an increase of 1.8 lb. on the gauge. It is therefore assumed that the friction on so small a piston in sand need not be considered as a material factor in the experiments made.

The piston was plunged into clear water, and it was found that the pressure required to lift it was about 4 lb. The cap was then taken off, a depth of about 2 in. of sand was placed in the bottom of the chamber, and then the piston was set in place and surrounded by sand to a depth of some 6 in., water being added so that the sand was completely saturated. This was allowed to stand until it had regained the stability of ordinary sand in place, whereupon the cap with the collar bearing was set in place over the piston, the machine was coupled up, and the pump was started. A series of four experiments, extending over a period of two or three days, gave the following results:

Test 1.—The piston began to move at a pressure of 25 lb. The pressure gradually dropped to 7½ lb., at which point, apparently, it came out of the sand, and continued at 7½ lb. during the remainder of the test.

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Test 2.—The piston was plunged back into the sand, without removing the cap, and allowed to stand for about 2 hours. No attempt was made to pack the sand or to see its condition around the piston, it being presumed, however, that it had reasonable time to get a fair amount of set. At slightly above 20 lb. the piston began to move, and as soon as a pocket of water accumulated behind the piston the pressure immediately dropped to 9 lb. and continued at this point until it came out of the sand.

Test 3.—The piston was plunged into the sand and hammered down without waiting for the sand to come to a definite set. In this case the initial pressure shown by the gauge was 17½ lb., which immediately dropped to 8 lb. as soon as the piston had moved sufficiently far to allow water to accumulate below it.

Test 4.—The cap was again removed, the piston set up in place, the sand compacted around it in approximately the same condition it would have had if the sand had been in place underground; the cap was then set in place and, after an hour, the pump was started. The pressure registered was 25 lb. and extended over a period of several seconds before there was any movement in the piston. The piston responded finally without any increase of pressure, and, after lifting an inch or two, the pressure gradually dropped to 10 lb., where it remained until the piston came out of the sand.

The sum and average of these tests shows a relation of 22 lb. for the piston in sand to about 8½ lb. as soon as the volume of water had accumulated below it, which would correspond very closely to a sand containing 40% of voids, which was the characteristic of the sand used in this experiment.

The conclusions from this experiment appear to be absolutely final in illustrating the pressure due to water on a tunnel buried in sand, either on the arch above or on the sides or bottom, as well as the buoyant effect upon the tunnel bottom under the same conditions.

While the apparatus would have to be designed and built on a much larger scale in order to measure accurately the pressures due to sands and earths of varying characteristics, it appears to be conclusive in showing the principle, and near enough to the theoretical value to be taken for practical purposes in designing structures against water pressures when buried in sand or earth.

It should be carefully noted that the friction of the water through sand, which is always a large factor in subaqueous construction, is

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virtually eliminated here, as the water pressure has to be transmitted only some 6 or 8 in. to actuate the base of the piston, whereas in a tunnel only half submerged this distance might be as many feet, and would be a considerable factor.

It should be noted also that although the area subject to pressure is diminished, the pressure on the area remaining corresponds to the full hydrostatic head, as would be shown by the pressure on an air gauge required to hold back the water, except, of course, as it may be diminished more or less by friction.

The writer understands that experiments of a similar nature and with similar apparatus have been tried on clays and peats with results considerably higher; that is, in one case, there was a pressure of 40 lb. before the piston started to move.

The following is given, in part, as an analysis and explanation of the above experiments and notes:

It is well known that if lead be placed in a hydraulic press and subjected to a sufficient pressure it will exhibit properties somewhat similar to soft clay or quicksand under pressure. It will flow out of an orifice or more than one orifice at the same pressure. This is due to the fact that practically voids do not exist and that the pressure is so great, compared with the molecular cohesion, that the latter is virtually nullified. It is also theoretically true that solid stone under infinitely high pressure may be liquefied. If in the cylinder of a hydraulic press there be put a certain quantity of cobblestones, leaving a clearance between the top of the stone and the piston, and if this space, together with the voids, be filled with water and subjected to a great pressure, the sides or the walls of the cylinder are acted on by two pressures, one almost negligible, where they are in contact with the stone, restraining the tendency of the stone to roll or slide outward, and the other due to the pressure of the water over the area against which there is no contact of stone. That this area of contact should be deducted from the pressure area can be clearly shown by assuming another cylinder with cross-sticks jammed into it, as shown in Fig. 10. A glance at this figure will show that there is no aqueous pressure on the walls of the cylinder with which the ends of the sticks come in contact and the loss of the pressure against the walls due to this is equal to the least sectional area of the stick or tube either at the point of contact or intermediate thereto.

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Following this reasoning, in Fig. 11 it is found that an equivalent area may be deducted covering the least area of continuous contact of the cobblestones, as shown along the dotted lines in the right half of the figure. Returning, if, when the pressure is applied, an orifice be made in the cylinder, the water will at once flow out under pressure, allowing the piston to come in contact with the cobblestones. If the flow of the water were controlled, so as to stop it at the point where the stone and water are both under direct pressure, it would be found that the pressures were totally independent of each other. The aqueous pressure, for instance, would be equal at every point, while the pressure on the stone would be through and along the lines of contact. If this contact was reasonably well made and covered 40% of the area, one would expect the stone, independently of the water, to

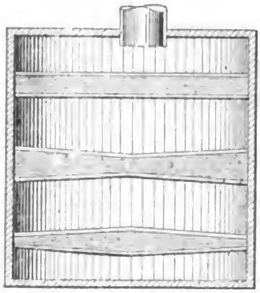

FIG. 10.

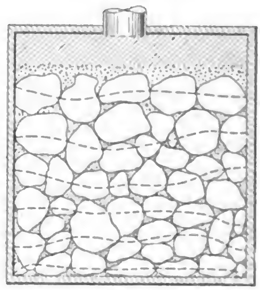

FIG. 11.

stand 40% of the pressure which a full area of solid stone would stand. If this pressure should be enormously increased after excluding the water, it would finally result in crushing the stone into a solid mass; and if the pressure should be increased indefinitely, some theoretical point would be reached, as above noted, where the stone would eventually be liquefied and would assume liquid properties.

The same general reasoning applies to pure sand, sand being in effect cobblestones in miniature. In pressing the piston down on dry sand it will be displaced into every existing abnormal void, but will be displaced into these voids rather than pressed into them, in the true definition of the word, and while it would flow out of an orifice in the sides or bottom, allowing the piston to be forced down as in a sand-jack, it would not flow out of an orifice in the top of the piston, except under pressures so abnormally high as to make the mass theoretically

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aqueous. If the positions of cylinder and piston be reversed, the piston pointing vertically upward and the sand "bled" into an orifice in or through it, the void caused by the outflow of this sand would be filled by sand displaced by the piston pressing upward rather than by sand from above.

It was the knowledge of this principle which enabled the contractors to jack up successfully the roof of a long section of the cast-iron lined tubes under Joralemon Street in Brooklyn, in connection with the reconstruction of the Battery tubes at that point, the method of operation, as partly shown in Fig. 2, Plate XXVIII, being to cut through a section of the roof, 4 by 10 ft. in area, through which holes were drilled and through which again the sand was "bled," heavy pressure being applied from below through the medium of hydraulic jacks. By a careful manipulation of both these operations, sections of the roof of the above dimensions were eventually raised the required height of 30 in. and permanently braced there in a single shift.

If water in excess be put into a cylinder containing sand, and pressure be applied thereto, the water, if allowed to flow out of an orifice, will carry with it a certain quantity of sand, according to the velocity, and the observation of this might easily give rise to the erroneous impression that the sand, as well as the water, was flowing out under pressure, and, as heretofore stated, has caused many engineers and contractors to apply the term "quicksand" to any sand flowing through an orifice with water.

Sand in its natural bed always contains some fine material, and where this is largely less than the percentage of voids, it has no material effect on the pressure exerted by the sand with or without water, as above noted. If, however, this fine material be largely in excess of the voids, it allows greater initial compression to take place when dry, and allows to be set up a certain amount of hydraulic action when saturated. If the base of the material be sand and the fill be so-called quicksand in excess of the voids, pressure will cause the quicksand to set up hydraulic action, and the action of the piston will appear to be similar to that of a piston acting on purely aqueous material.

Just here the writer desires to protest against considering semi-aqueous masses, such as soupy sands, soft concrete, etc., as exerting hydrostatic pressure due to their weight in bulk, instead of to the

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specific gravity of the basic liquid. For instance, resorting again to the illustration of cubes and spheres, it may be assumed that a cubical receptacle has been partly filled with small cubes of polished marble, piled vertically in columns. When this receptacle is filled with liquid around the piles of cubes there will be no pressure on the sides except that due to the hydrostatic pressure of the water at 62½ lb. The bottom, however, will resist a combined pressure due to the water and the weight of the cubes. Again, assume that the receptacle is filled with small spheres, such as marbles, and that water is then poured in. The pressure due to the weight of the solids on the bottom is relieved by the loss in weight of the marbles due to the water, and also to the tendency of the marbles to arch over the bottom, and while the pressure on the sides is increased by this amount of thrust, the aqueous pressure is still that of a liquid at 62½ lb., and it is inconceivable that some engineers, in calculating the thrust of aqueous masses, speak of it as a liquid weighing, say, 120 or 150 lb. per cu. ft.; as well might they expect to anchor spherical copper floats in front of a bulkhead and expect the hydrostatic pressure against this bulkhead to be diminished because the actual volume and weight of the water directly in front of the bulkhead has been diminished. Those who have had experience in tying narrow deep forms for concrete with small wires or bolts and quickly filling them with liquid concrete, must realize that no such pressures are ever developed as would correspond to liquids of 150 lb. per cu. ft. If the solid material in any liquid is agitated, so that it is virtually in suspension, it cannot add to the pressure, and if allowed to subside it acts as a solid, independently of the water contained with it; although the water may change somewhat the properties of the material, by increasing or changing its cohesion, angle of repose, etc. That is, in substance, those particles which rest solidly on the bottom and are in contact to the top of the solid material, do not derive any buoyancy from the water, while those particles not in contact with the bottom directly or through other particles, lose just so much weight through buoyancy. If, then, the vertical depth of the earthy particles or sand above the bottom is so small that the arching effect against the sides is negligible, the full weight of the particles in contact, directly or vicariously, with the bottom acts as pressure on the bottom, while the full pressure of the water acts through the voids or on them, or is transmitted through material in contact with the bottom.

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Referring now to materials such as clays, peats, and other soft or plastic materials, it is idle to assume that these do not possess pressure-resisting and arching properties. For instance, a soft clay arch of larger dimensions, under the condition described early in this paper, would undoubtedly stand if the rods supporting the intrados of the arch were keyed back to washers covering a sufficiently large area.

The fact that compressed air can be used at all in tunnel work is evidence that semi-aqueous materials have arching properties, and the fact that "blows" usually occur in light cover is further evidence of it.

When air pressure is used to hold back the water in faces of large area, bracing has to be resorted to. This again shows that while full hydrostatic pressure is required to hold back the water, the pressure of the earth is in a measure independent of it.

In a peaty or boggy material there is a condition somewhat different, but sufficiently allied to the soft clayey or soupy sands to place it under the same head in ordinary practice. It is undoubtedly true that piles can be driven to an indefinite depth in this material, and it is also true that the action of the pile is to displace rather than compress, as shown by the fact of driving portions of the tunnels under the North River for long distances without opening the doors of the shield or removing any of the material. The case of filling in bogs or marshes, causing them to sink at the point of filling and rise elsewhere, is readily explained by the fact that the water is confined in the interstices of the material, admitting of displacement but no compression.

The application of the above to pressures over tunnels in materials of Class A is that the sand or solid matter is virtually assumed to be a series of columns with their bases in such intimate contact with the tunnel roof that water cannot exert pressure on the tunnel or buoyancy on the sand at the point of contact, and that if these columns are sufficiently deep to have their upper portions wholly or partly carried by the arching or wedging action, the pressure of any water on their surfaces is not transferred to the tunnel, and the only aqueous pressure is that which acts on the tunnel between the assumed columns or through the voids.

Let l = exterior width of tunnel,

δ = depth of cover, as :

$D_W =$ depth, water to roof,

$D_E =$ " earth to roof,

$D_X =$ " of cover of earth necessary to arching stability,

that is:

$$D_X = \frac{l}{2}\left(\tan.\left\{\frac{90^\circ - \phi}{2}\right\} + \phi\right) = \frac{l}{2}\tan.\left(45^\circ + \frac{\phi}{2}\right),$$

where $\phi =$ angle of repose,

and $D_W > D_E > D_X$.

Then the pressure on any square foot of roof, as V_P, as at the base of any vertical ordinate, as 9 in Fig. 2, $= V_O$.

$W_E =$ weight per cubic foot of earth (90 lb.),

$W_W =$ " " " " " water ($62\frac{1}{2}$ lb.), we have

$$V_P = V_O \times W_E + D_W \times W_W \times 0.40 = V_O \times 90 + D_W \times 62\tfrac{1}{2} \times 0.4$$
$$= V_O\ 90 + D_W \times 25.$$

And for horizontal pressure:

$P_h =$ the horizontal pressure at any abscissa (10), Fig. 2, $= A_{10}$ at depth of water D_{W1} is

$$P_h = \frac{A_{10} \times 90}{\tan.\ \phi} + D_{W1} \times 62\tfrac{1}{2} \times 0.4 = \frac{A_{10} \times 90}{\tan.\ \phi} + D_{W1} \times 25.$$

The only question of serious doubt is at just what depth the sand is incapable of arching itself, but, for purposes of safety, the writer has put this at the point, F, as noted above, $= D_X$, although he believes that experiments on a large scale would show it to be nearer 0.67 D_X, above which the placing of additional back-fill will lighten the load on the structure.

We have, then, for $D_E < D_X$, the weight of the total prism of the earth plus the water in the voids, plus the added pressure of the water above the earth prism, that is:

The pressure per square foot at the base of any vertical ordinate $= V_P$

$$V_P = D_E \times 90 + D_E \times 62\tfrac{1}{2} \times 0.40 + (D_W - D_E) \times 62\tfrac{1}{2}.$$

To those who may contend that water acting through so shallow a prism of earth would exert full pressure over the full area of the tunnel, it may be stated that the water cannot maintain pressure over the whole area without likewise giving buoyancy to the sand previously assumed to be in columns, in which case there is the total weight of the water plus the weight of the prism of earth, less its buoyancy in water, that is

$$V_P = D_W \times 62\tfrac{1}{2} + D_E \times (90 - 62\tfrac{1}{2}),$$

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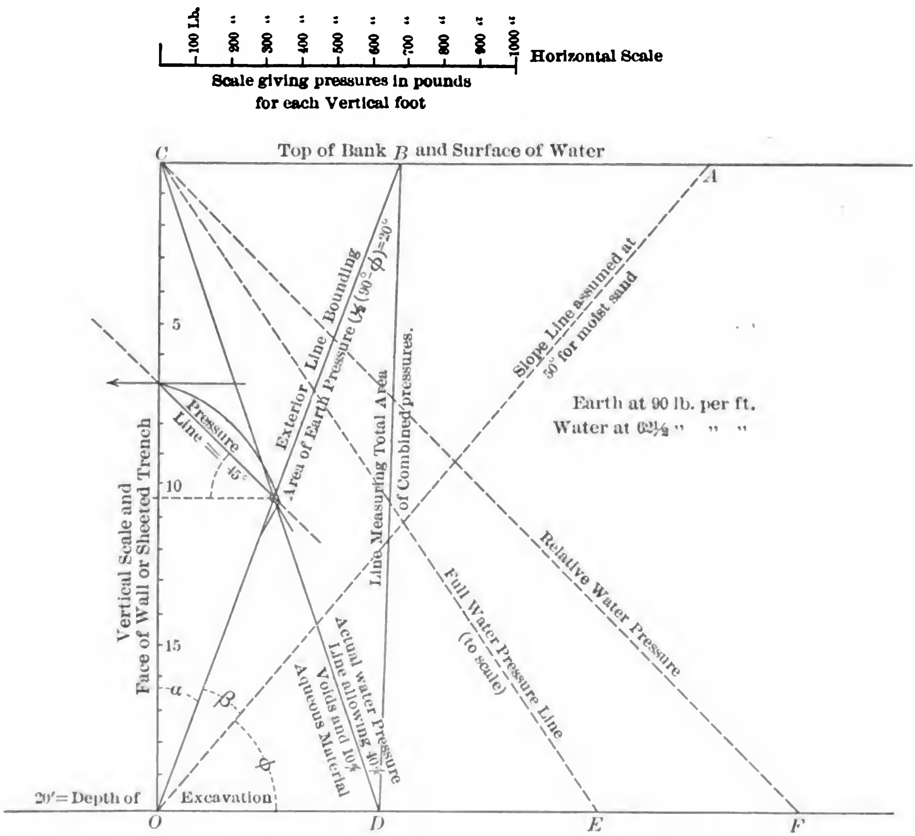
which, by comparison with the former method, would appear to be less safe in its reasoning.

Next is the question of pressure against a wall or braced trench for materials under Class A. The pressure of sand is first calculated independently, as shown in Fig. 6. Reducing this to a basis of 100 lb.



COMBINED EARTH AND WATER PRESSURES.

Fig. 12.

for each division of the scale measured horizontally, as shown, gives the line, *B O*, Fig. 12, measuring the outside limit of pressure due to the earth, the horizontal distance at any point between this line and the vertical face equalling the pressure against that face divided by the tangent of the angle of repose, which in this case is assumed to be 45°, equalling unity. If the water pressure line, *C F*, is drawn, it

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shows the relative pressure of the water. In order to reduce this to the scale of 100 lb. horizontal measurement, the line, *C E*, is drawn, representing the water pressure to scale, that is, so that each horizontal measurement of the scale gives the pressure on the face at that point; and, allowing 50% for voids, halving this area gives the line, *C D*, between which and the vertical face any horizontal line measures the water pressure. Extending these pressure areas where they overlap gives the line, *B D*, which represents the total pressure against the face, measured horizontally.

Next, as to the question of buoyancy in Class A materials. If a submerged structure rests firmly on a bottom of more or less firm sand, its buoyancy, as indicated by the experiments, will only be a percentage of its buoyancy in pure water, corresponding to the voids in the sand. In practice, however, an attempt to show this condition will fail, owing to the fact that in such a structure the water will almost immediately work under the edge and bottom, and cause the structure to rise, and the test can only be made by measuring the difference in uplift in a heavier-than-water structure, as shown in Experiment No. 5. For, if a structure lighter than the displaced water be buried in sand sufficiently deep to insure it against the influx of large volumes of water below, it will not rise. That this is not due entirely to the friction of the solid material on the sides has been demonstrated by the observation of subaqueous structures, which always tend to subside rather than to lift during or following disturbance of the surrounding earth.

The following is quoted from the paper by Charles M. Jacobs, M. Am. Soc. C. E., on the North River Division of the Pennsylvania Railroad Tunnels:*

"There was considerable subsidence in the tunnels during construction and lining, amounting to an average of 0.34 ft. between the bulkhead lines. This settlement has been constantly decreasing since construction, and appears to have been due almost entirely to the disturbances of the surrounding materials during construction. The silt weighs about 100 lb. per cu. ft. * * * and contains about 38% of water. It was found that whenever this material was disturbed outside the tunnels a displacement of the tunnels followed."

This in substance confirms observations made in the Battery tubes that subsidence of the structure followed disturbance of the outside

* *Transactions*, Am. Soc. C. E., Vol. LXVIII, pp. 58–60.

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material, although theoretically the tubes were buoyant in the aqueous material.

The writer would urge, however, that, in all cases of submerged structures only partially buried in solid material, excess weighting be used to cover the contingencies of vibration, oscillation, etc., to which such structures may be subjected and which may ultimately allow leads of water to work their way underneath.

On the other hand, he urges that, in cases of floor areas of deeply submerged structures, such as tunnels or cellars, the pressure to be resisted should be assumed to be only slightly in excess of that corresponding to the pressure due to the water through the voids.

The question of pressure, etc., in Class B, or semi-aqueous materials will be considered next. Of these materials, as already shown, there are two types: (*a*) sand in which the so-called quicksand is largely in excess of any normal voids, and (*b*) plastic and viscous materials. The writer believes that these materials should be treated as mixtures of solid and watery particles, in the first of which the quicksand, or aqueous portion, being virtually in suspension, may be treated as water, and it must be concluded that the action here will be similar to that of sand and pure water, giving a larger value to the properties of water than actually exists. If, for instance, it should be found that such a mixture contained 40% of pure water, the writer would estimate its pressure on or against a structure as (*a*) that of a moist sand standing at a steep angle of repose, and (*b*) that of clear water, an allowance of 60% of the total volume being assumed, and the sum of these two results giving the total pressure. Until more definite data can be obtained by experiments on a larger scale, this assumed value of 60% of the total volume for the aqueous portion may be taken for all conditions of semi-aqueous materials, except, of course, where the solid and aqueous particles may be clearly defined, the pressures being computed as described in the preceding pages.

As to the question of pure quicksand (if such there be) and other aqueous materials of Class C, such as water, oil, mercury, etc., it has already been shown that they are to be considered as liquids of their normal specific gravity; that is, in calculating the air pressure necessary to displace them, one should consider their specific gravity only, as a factor, and not the total weight per volume including any impurities which they might contain undissolved.

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In order to have a clearer conception of aqueous and semi-aqueous materials and their action, they must be viewed under conditions not ordinarily apparent. For instance, ideas of so-called quicksand are largely drawn from seeing structures sinking into it, or from observing it flowing through voids in the sheeting or casing. The action of sand and water under pressure is viewed during or after a slump, when the damage is being done, or has been done, whereas the correct view-point is under static conditions, before the slump takes place.

The following is quoted from the report of Mr. C. M. Jacobs, Chief Engineer of the East River Gas Tunnel, built in 1892-93:

"We found that the material which had heretofore been firm or stiff had, under erosion, obtained a soup-like consistency, and that a huge cavity some 3 ft. wide and 26 ft. deep had been washed up toward the river bed."

This would probably be a fair description of much of the material of this class met with in such work, if compressed air had not been used. The writer believes that in soft material surrounding submerged structures the water actually contained in the voids is not infrequently, after a prolonged period of rest, cut off absolutely from its sources of pressure and that contact with these sources of pressure will not again be resumed until a leak takes place through the structure; and, even when there is a small flow or trickling of water through such material, it confines itself to certain paths or channels, and is largely excluded from the general mass.

The broad principle of the bearing power of soil has been made the subject of too many experiments and too much controversy to be considered in a paper which is intended to be a description of experiments and observed data and notes therefrom. The writer is of the opinion, however, that entirely too little attention has been given to this bearing power of the soil; that while progress has been made in our knowledge of all classes of materials for structures, very little has been done which leads to any real knowledge of the material on which the foundation rests. For instance, it is inconceivable that 1 or 2 tons may sometimes be allowed on a square foot of soft clay, while the load on firm gravel is limited to from 4 to 6 tons. The writer's practical observations have convinced him that it is frequently much safer to put four times 6 tons on a square foot of gravel than it is to put one-fourth of 2 tons on a square foot of soft clay.

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In connection with the bearing power of soil, the writer also believes that too little study has been given to the questions of the lateral pressure of earth, and he desires to quote here from some experiments described in a book* published in England in 1876, to which his attention has recently been called. This book appears to have been intended for young people, but it is of interest to note the following quotations from a chapter entitled "Sand." This chapter begins by stating that:

"During the course of a lecture on the Suez Canal by Mr. John H. Pepper, which was delivered nightly by him at the Polytechnic Institute in London, he illustrated his lecture by some experiments designed to exhibit certain properties of sand, which had reference to the construction of the Suez Canal, and it is stated that though the properties in question were by no means to be classed among recent discoveries, the experiments were novel in form and served to interest the public audience."

Further quotation follows:

"When the Suez Canal was projected, many prophesied evil to the undertaking, from the sand in the desert being drifted by the wind into the canal, and others were apprehensive that where the canal was cut through the sand the bottom would be pushed up by the pressure on the banks * * *.

"The principle of lateral pressure may now be strikingly illustrated by taking an American wooden pail and, having previously cut a large circular hole in the bottom, this is now covered with fine tissue paper, which should be carefully pasted on to prevent the particles of sand from flowing through the small openings between the paper and the wood * * * and being placed upright and rapidly filled with sand, it may be carried about by the handle without the slightest fear of the weight of the sand breaking through the thin medium. * * *

"Probably one of the most convincing experiments is that which may be performed with a cylindrical tube 18 in. long and 2 in. in diameter, open at both ends. A piece of tissue paper is carefully pasted on one end, so that when dry no cracks or interstices are left. The tube is filled with dry sand to a height of say 12 in. In the upper part is inserted a solid plug of wood 12 in. long and of the same or very nearly the same diameter as the inside of the tube, so that it will move freely up and down like the piston of an air pump. The tube, sand, and piston being arranged as described, may now be held by an assistant and the demonstrator, taking a sledge hammer,

* "Discoveries and Inventions of the Nineteenth Century," by Robert Routledge, Assistant Examiner in Chemistry and in Natural Philosophy to the University of London.

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may proceed to strike steadily on the end of the piston and, although the paper will bulge out a little, the force of the blow will not break it.

"If the assistant holding the tube allows it to jerk or rebound after each blow of the hammer, the paper may break, because air and sand are driven down by the succeeding blow, and therefore it must be held steadily so that the piston bears fairly on the sand each time.

"A still more conclusive and striking experiment may be shown with a framework of metal constructed to represent a pail, the sides of which are closed up by pasting sheets of tissue paper inside and over the lower part. As before demonstrated, when a quantity of sand is poured into the pail the tissue paper casing at the bottom does not break, but if a sufficient quantity is used the sides formed of tissue paper bulge out and usually give way in consequence of the lateral pressure exerted by the particles of sand."

The writer has made the second experiment noted, with special apparatus, and finds that with tissue paper over the bottom of a 2-in. pipe, 15 in. long, about 12 in. of sand will stand the blow of a heavy sledge hammer, transmitted through a wooden piston, at least once and sometimes two or three times, while heavy blows given with a lighter hammer have no effect at all. That this is not due in any large measure to inertia can be shown by the fact that more than 200 lb. can safely be put on top of the wooden piston. It cannot be accounted for entirely by the friction, as the removal of the paper allows the sand to drop in a mass. The explanation is that the pressure is transmitted laterally to the sides, and as the friction is directly proportional to the pressure, the load or effect of the blow is carried by the proportional increase in the friction, and any diaphragm which will carry the direct bottom load will not have its stresses largely increased by any greater loading on top.

The writer believes that experiments will show that in a sand-jack the tendency will be for the sides to burst rather than the bottom, and that the outflow from an orifice at or near the bottom is not either greatly retarded or accelerated by ordinary pressure on top. The occurrence of abnormal voids, however, causes the sand to be displaced into them.

The important consideration of this paper is that all the experiments and observations noted point conclusively to the fact that pressure is transmitted laterally through ground, most probably along or nearly parallel to the angles of repose, or in cases of rock or stiff material, along a line which, until more conclusive experiments are

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made, may be taken as a mean between the horizontal and vertical, or approximately 45 degrees. There is no reason to believe that this is not the case throughout the entire mass of the earth, that each cubic foot, or yard, or mile is supported or in turn supports its neighboring equivalent along such lines. The theory is not a new one, and its field is too large to encompass within the limits of a single paper, but, for practical purposes, and within the limited areas to which we must necessarily be confined, the writer believes it can be established beyond controversy as true. Certain it is that no one has yet found, in ground free from water pressure or abnormal conditions, any evidence of greater pressure at the bottom of a deep shaft or tunnel than that near the surface. Pressures due to the widening of mines beyond the limits of safety must not be taken as a controversion of this statement, as all arches have limits of safety, more especially if the useless material below the theoretical intrados is only partly supported, or is allowed to be suspended from the natural arch.

The writer believes, also, that the question of confined foundations, in contradistinction to that of the spreading of foundations, may be worthy of full discussion, as it applies to safe and economical construction, and he offers, without special comment, the following observations:

He has found that, in soft ground, results are often obtained with small open caissons sunk to a depth of a few feet and cleaned out and filled with concrete, which offer much better resistance than spreading the foundation over four or five times the equivalent area.

He has found that small steel piles and coffer-dams, from 1-ft. cylinders to coffer-dams 4 or 5 ft. square, sunk to a depth of only 1 or 2 ft. below adjacent excavations in ordinary sand, have safely resisted loads four or five times as great as those usually allowed.

He believes that short cylinders, cleaned out and filled with concrete, or coffer-dams of short steel piling with the surface cleaned out to a reasonable depth and filled with concrete horizontally reinforced, will, in many instances, give as good results as, and, in most cases, very much better than, placing the foundation on an equivalent number of small long piles or a proportionately greater spread of foundation area, the idea being that the transmission of pressure to the sides of the coffer-dam will not only confine the side thrust, but will also transfer the loading in mass to a greater depth where the resistance to

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lateral pressure in the ground will be more stable; that is, the greater depth of foundation is gained without the increased excessive loading, or necessity for deep excavation.

As to the question of the bearing value and friction on piles, the writer believes that while the literature on engineering is full of experimental data relating to friction on caissons, there is little to show the real value of friction on piles. The assumption generally made of an assumed bearing value, and the deduction therefrom of a value for the skin friction is fallacious. Distinction, also, is not made, but should be clearly drawn between skin friction, pure and simple, on smooth surfaces, and the friction due to pressure. Too often the bearing value on irregular surfaces as well as the bearing due to taper in piles, and lastly the resistance offered by binding, enter into the determination of so-called skin friction formulas. The essential condition of sinking a caisson is keeping it plumb; and binding, which is another way of writing increased bearing value, will oftentimes be fatal to success.

The writer believes that a series of observations on caissons sunk plumb under homogeneous conditions of ground and superficial smoothness will show a proportional increase of skin friction per square foot average for each increase in the size of caissons, as well as for increase of depth in the sinking up to certain points, where it may finally become constant, as will be shown later. The determination of the actual friction or coefficient of friction between the surfaces of the pile and the material it encounters, is not difficult to determine. In sand it is approximately 40% of the pressure for reasonably smooth iron or steel, and 45% of the pressure for ordinary wood surfaces. If, for instance, a long shaft be withdrawn vertically from moulding sand, the hole may remain indefinitely as long as water does not get into it or it does not dry out. This is due to the tendency of the sand to arch itself horizontally over small areas. The same operation cannot be performed on dry sand, as the arching properties, while protecting the pile from excessive pressure due to excessive length, will not prevent the loose sand immediately surrounding the pile from exerting a constant pressure against the pile, and it is of this pressure that 45% may be taken as the real value of skin friction on piles in dry sand.

In soft clays or peats which are displaced by driving, the tendency of this material to flow back into the original space causes pressure, of

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which the friction will be a measured percentage. In this case, however, the friction itself between the material and the clays or peat is usually very much less than 40%, and it is for this reason that piles of almost indefinite length may be driven in materials of this character without offering sufficient resistance to be depended on, as long as no good bearing ground is found at the point.

If this material is under water, and is so soft as to be considered semi-aqueous, the pressure per square foot will increase in diminishing proportion to the depth, and the pressure per area will soon approach and become a constant, due to the resistance offered by the lateral arching of the solid material; whereas, in large circular caissons, or caisson shafts, where the horizontal arching effect is virtually destroyed, or at least rendered non-effective until a great depth is reached, the pressure must necessarily vary under these conditions proportionately to the depth and size of the caisson in semi-aqueous material. On the other hand, in large caisson shafts, especially those which are square, the pressure at the top due to the solid material will also increase proportionately to the depth, as already explained in connection with the pressures of earth against sheeting and retaining walls.

The writer believes that the pressure on these surfaces may be determined with reasonable accuracy by the formulas already given in this paper, and with these pressures, multiplied by the coefficient of friction determined by the simplest experiment on the ground, results may be obtained which will closely approximate the actual friction on caissons at given depths. The friction on caissons, which is usually given at from 200 to 600 lb. per sq. ft., is frequently assumed to be the same on piles 12 in. or less in diameter, whereas the pressures on these surfaces, as shown, are in no way comparable.

The following notes and observations are given in connection with the skin friction and the bearing value of piles:

The writer has in his possession a copy of an official print which was recently furnished to bidders in connection with the foundation for a large public building in New York City. The experiments were made on good sand at a depth of approximately 43 ft. below water and 47 ft. below an adjacent excavation. In this instance a 16-in. pipe was sunk to the depth stated, cleaned out, and a 14-in. piston connected to a 10-in. pipe was inserted and the ground at the bottom of the 16-in. pipe subjected to a loading approximating 28 tons per sq. ft.

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After an initial settlement of nearly 3 in., there was no further settlement over an extended period, although the load of 28 tons per sq. ft. was continued.

In connection with some recent underpinning work, 14-in. hollow cylindrical piles 6 ft. long were sunk to a depth of 6 ft. with an ordinary hand-hammer, being excavated as driven. These piles were then filled with concrete and subjected to a loading in some cases approximating 60 tons. After a settlement ranging from 9 to 13 in., no further settlement took place, although the loading was maintained for a considerable period.

In connection with some other pile work, the writer has seen a 10-in. pipe, $\frac{3}{8}$ in. thick, 4 ft. below the bottom of an open cylinder, at a depth of about 20 ft., sustain in gravel and sand a load approximating 50 tons when cleaned out to within 2 ft. of the bottom.

He has seen other cylindrical piles with a bearing ring of not more than $\frac{3}{4}$ in. resting on gravel at a depth of from 20 to 30 ft., cleaned out practically to the bottom, sustain a measured load of 60 tons without settlement.

As to skin friction in sand, a case came under his observation wherein a 14-in. hollow cylindrical pile which had stood for 28 days at a depth of about 30 ft. in the sand, was cleaned out to its bottom and subjected to hydraulic pressure, measured by a gauge, and sunk 2 ft. into the sand without any pressure being registered on the gauge. It should be explained, however, that the gauge could be subjected to a pressure of 250 lb., equal to a total pressure of 7 000 lb. on the piston of the jack without registering, which corresponded, assuming it all as skin friction, to a maximum of not more than 78 lb. per sq. ft., but it should be noted that this included bearing value as well, and that the pressure was very far from 7 000 lb., in all probability, at the beginning of the test.

In the case of the California stove-pipe wells driven by the Board of Water Supply on Long Island, the writer is informed that one of these tubes, 12 in. in diameter, was sunk to a depth of 850 ft. In doing this work the pile was excavated below the footing with a sand pump and was then sunk by hydraulic pressure. Assuming the maximum capacity of the jacks at 100 tons, which is not probable, the skin friction could not have amounted to more than 75 lb. per sq. ft. It cannot be assumed in this case that the excavation of the material

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PLATE XXIX.
TRANS. AM. SOC. CIV. ENGRS.
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MEEM ON
PRESSURE, RESISTANCE, AND STABILITY
OF EARTH.

FIG. 1.—A 14-GAUGE, 14-IN., HOLLOW (NON-TELESCOPIC), CALIFORNIA STOVE-PIPE PILE WHICH MET IMPENETRABLE MATERIAL.

FIG. 2.—CHENOWETH PILE, PENETRATING HARD MATERIAL.

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below the pile relieved the skin itself of some of its friction, as the operation consumed more than 6 weeks, and, even if excess material was removed, it is certain that a large percentage of it would have had time to adjust itself before the operation was completed.

In connection with this, the writer may call attention to the fact that piles driven in silt along the North River, and in soft material at other places, are sometimes 90 ft. in length, and even then do not offer sufficient resistance to be depended on for loading. This is due to the fact that the end of the pile does not bear in good material.

The relation between bearing value and skin friction on a pile, where the end bearing is in good material, is well shown by a case where a wooden pile* struck solid material, was distorted under the continual blows of the hammer, and was afterward exposed. It is also shown in the case of a 14-in. California stove-pipe pile, No. 14 gauge, the point of which met firm material. The result, as shown by Fig. 1, Plate XXIX, speaks for itself. Fig. 2, Plate XXIX, shows a Chenoweth pile which was an experimental one driven by its designer. This pile, after getting into hard material, was subjected to the blow of a 4 000-lb. hammer falling the full length of the pile-driver, and the only result was to shatter the head of the pile, and not cause further penetration. Mr. Chenoweth has stated to the writer that he has found material so compact that it could not be penetrated with a solid pile—either with or without jetting—which is in line with the writer's experience.

The writer believes that the foregoing notes will show conclusively that the factor to be sought in pile work is bearing value rather than depth or skin friction, and, however valuable skin friction may be in the larger caissons, it cannot be depended on in the case of small piles, except in values ranging from 25 to 100 lb. per sq. ft.

In conclusion, he desires to thank the following gentlemen, who have contributed to the success of the experiments noted herein: Mr. James W. Nelson, of Richard Dudgeon, New York; Mr. George Noble, of John Simmons and Company, New York; and Mr. Pendleton, of Hindley and Pendleton, Brooklyn, N. Y.; all of whom have furnished apparatus for the experiments and have taken an interest in the results. And lastly, he desires especially to thank Mr. F. L. Cranford, of the Cranford Company, for men and material with which to make

* *Engineering News*, January 15th, 1909.

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the experiments and without whose co-operation it would have been impracticable for the writer to have made them.

Throughout this paper the writer has endeavored, as far as possible, to deduce from his observations and from the observations of others, as far as he has been able to obtain them, practical data and formulas which may be of use in establishing the relationship between the pressure, resistance, and stability of earths; and, while he does not wish to dictate the character of the discussion, he does ask that those who have made observations of a similar character or who have available data, will, as far as possible, contribute the same to this discussion. It is only by such observations and experiments, and deductions therefrom, that engineers may obtain a better knowledge of the handling of such materials.

The writer believes that too much has been taken for granted in connection with earth pressures and resistance; and that, far too often, observations of the results of natural laws have been set down as phenomena. He believes that, both in experimenting and observing, the engineer will frequently find what is being looked for or expected and will fail to see the obvious alternative. He may add that his own experiments and observations may be criticized for the same reason, and he asks, therefore, that all possible light be thrown on this subject. A comparative study of much of our expert testimony or of the plans of almost any of the structures designed in connection with their bearing upon earth, or resistance to earth pressure, will show that under the present methods of interpretation of the underlying principles governing the calculations and designs relating to such structures, the results vary far too widely. Too much is left to the judgment of the engineer, and too frequently no fixed standards can be found for some of the most essential conditions.

Until the engineer can say with certainty that his calculations are reasonably based on facts, he is forced to admit that his design must be lacking, either in the elements of safety, on the one hand, or of economy, on the other, and, until he can give to his client a full measure of both these factors in fair proportion, he cannot justly claim that his profession has reached its full development.

Table 1 gives approximate calculations of pressures on two types of tunnels and on two heights of sheeted faces or walls, due to four widely varying classes of materials.

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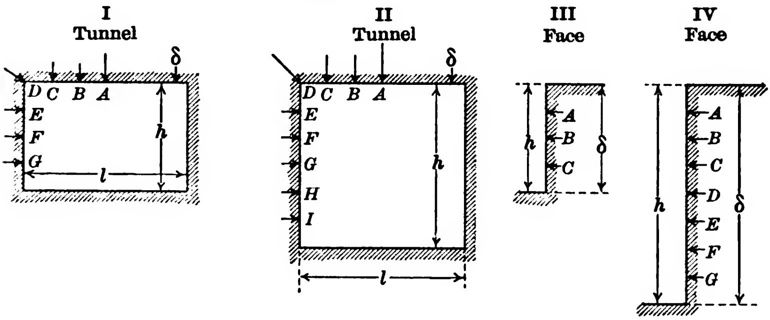
TABLE 1.—Pressures on Typical Structures Under Varying Assumed Conditions.



Key to Table of Pressures, etc.

h = exterior height, l = exterior width,

$\left\{ \begin{array}{l} \delta = \text{depth of cover, that is,} \\ D_E = \text{earth, and } D_W = \text{water depth,} \end{array} \right.$

ϕ = angle of repose, and, for tunnels $D_W > D_E >$ a depth

$$= \frac{l}{2}\left(45° + \frac{\phi}{2}\right)$$

W_E = weight of 1 cu. ft. of earth = 90 lb.; W_W = weight of 1 cu. ft. of water = $62\frac{1}{2}$ lb.

Conditions: 1 = normal sand, 2 = dry sand, 3 = supersaturated firm sand with 40% of voids, 4 = supersaturated semi-aqueous material, 60% aqueous, that is, 60% water and aqueous material.

Combined assumed conditions.	h	l	ϕ	D_E	Combined assumed conditions.	h	l	ϕ	D_E	D_W
I_1	20	30	45°	40	I_3	20	30	50°	40	60
I_2	20	30	30°	40	I_4	20	30	40°	40	60
II_1	15	15	45°	40	II_3	15	15	50°	40	60
II_2	15	15	30°	40	II_4	15	15	40°	40	60
III_1	15	....	45°	15	III_3	15	....	50°	15	15
III_2	15	....	30°	15	III_4	15	....	40°	15	15
IV_1	30	....	45°	30	IV_3	30	....	50°	30	30
IV_2	30	....	30°	30	IV_4	30	....	40°	30	30

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TABLE 1.—(*Continued.*)

Approximate Pressures on Tunnels, per Square Foot.

Pressure, per square foot, at	I_1 Earth.	I_3 Earth.	I_3 Water.	I_3 Combined.	I_2 Earth.	I_4 Earth.	I_4 Water.	I_4 Combined.	II_1 Earth.	II_3 Earth.	II_3 Water.	II_3 Combined.	II_2 Earth.	II_4 Earth.	II_4 Water.	II_4 Combined.
A	3 240	3 690	1 500	5 190	2 325	2 880	2 250	5 130	1 485	1 755	1 500	3 255	1 035	1 305	2 250	3 555
B	2 745	3 105	1 500	4 605	1 845	2 385	2 250	4 635	1 305	1 485	1 500	2 985	945	1 170	2 250	3 420
C	2 160	2 475	1 500	3 975	1 350	1 800	2 250	4 050	1 125	1 215	1 500	2 715	810	990	2 250	3 240
D	450	540	1 500	2 040	450	450	2 250	2 700	405	405	1 500	1 905	540	450	2 250	2 700
E	360	360	1 625	1 985	450	450	2 438	2 888	405	405	1 625	2 030	540	450	2 438	2 888
F	270	270	1 750	2 025	450	360	2 626	2 986	360	360	1 750	2 110	540	450	2 626	3 076
G	225	225	1 875	2 100	360	270	2 814	3 084	315	315	1 875	2 190	360	360	2 814	3 174
H	....	....	....	....	....	....	....	....	180	225	2 000	2 225	180	180	3 000	3 180
I	....	....	....	....	....	....	....	....	90	110	2 175	2 285	135	135	3 188	3 323

Approximate Pressures on Sheeted Trench Faces or Walls.

Pressure per square foot at	III_1 Earth.	III_3 Earth.	III_3 Water.	III_3 Total earth and water.	III_2 Earth.	III_4 Earth.	III_4 Water.	III_4 Total earth and water.	IV_1 Earth.	IV_3 Earth.	IV_3 Water.	IV_3 Total earth and water.	IV_2 Earth.	IV_4 Earth.	IV_4 Water.	IV_4 Total earth and water.
A........	575	510	100	610	1 350	810	140	950	1 370	1 210	100	1 310	3 175	1 910	150	2 060
B........	400	350	190	540	900	540	260	800	1 170	1 030	200	1 230	2 700	1 610	290	1 900
C........	200	175	280	455	450	270	380	650	970	855	290	1 145	2 250	1 355	430	1 785
D........	....	....	....	....	....	....	....	....	775	680	370	1 050	1 800	1 100	570	1 670
E........	....	....	....	....	....	....	....	....	590	515	460	975	1 350	820	710	1 530
F........	....	....	....	....	....	....	....	....	400	350	560	910	900	540	860	1 400
G........	....	....	....	....	....	....	....	....	190	170	650	820	450	275	1 000	1 275

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DISCUSSION

T. KENNARD THOMSON, M. AM. SOC. C. E.—Although the author deserves great credit for the careful and thorough manner in which he has handled this subject, his paper should be labeled "Dangerous for Beginners," especially as he is an engineer of great practical experience; if he were not, comparatively little attention would be paid to his statements. The paper is dangerous because many will read only portions of it, or will not read it thoroughly. For instance, at the beginning, the author cites several experiments in which considerable force is required to start the lifting of a weight or plunger in sand and water and much less after the start. This reminds the speaker of the time when, as a schoolboy, he tried to pick up stones from the bottom of the river and was told that the "suction" was caused by atmospheric pressure. Mr. Thomson.

The inference is that tunnels, etc., in sand, etc., are not in any danger of rising, even though they are lighter than water. Toward the end of the paper, however, the author states that tunnels should be weighted, but he rather spoils this by stating that they should be weighted only enough to overcome the actual water pressure, that is, between the voids of the sand. It seems to the speaker that the only really safe way is to make the tunnel at least as heavy as the water displaced in order to prevent it from coming up, and to take other measures to prevent it from going down. The City of Toronto, Canada, formerly pumped its water supply through a 6-ft. iron pipe, buried in the sand under Toronto Bay and then under Toronto Island, with an intake in the deep water of the lake. During a storm a mass of seaweed, etc., was washed against the intake, completely blocking it, and although the man at the pumping station knew that something was wrong, he continued to pump until the water was drawn out of the pipe, with the result that about half a mile of the conduit started to rise and then broke at several places, thus allowing it to fill with water. Eventually, the city went down to bed-rock under the Bay for its water tunnel.

Another reason for calling this paper dangerous for beginners is that it is improbable that experienced engineers or contractors will omit the bracing at the bottom, although, since the paper was printed, a glaring instance has occurred where comparatively little bracing was put in the bottom of a 40-ft. cut, the result being a bad cave-in from the bottom, although all the top braces remained in place. Most engineers will agree that nearly every crib which has failed slipped out from the bottom, and did not turn over.

The objection to the angle of repose is that it is not possible to ascertain it for any material deposited by Nature. It could probably be ascertained for a sand bank deposited by Man, but not for an excava-

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Mr. Thomson. tion to be made in the ground, for it is known that nearly all earth, etc., has been deposited under great pressure, and is likely to be cemented together by clay, loam, roots, trees, boulders, etc., and differs in character every few feet.

A deep vertical cut can often be made, even in New York quicksand, from which the water has been drawn, and, if not subjected to jars, water, etc., this material will stand for considerable time and then come down like an avalanche, killing any one in its way. In such cases very little bracing would prevent the slide from starting, provided rain, etc., did not loosen the material.

The author, of course, treats dry and wet materials differently, but there are very few places where dry material is not likely to become wet before the excavation is completed.

In caisson work, if the caisson can be kept absolutely plumb, it can be sunk without having to overcome much friction, while, on the other hand, if it is not kept plumb, the material is more or less disturbed and begins to bind, causing considerable friction. The author claims that the pressure does not increase with the depth, but all caisson men will probably remember that the friction to be overcome per square foot of surface increases with the depth.

In calculating retaining walls, many engineers add the weight of the soil to the water, and calculate for from 90 to 100 lb. per cu. ft. The speaker is satisfied that in the so-called New York quicksand it is sufficient to use the weight of the water only. If the sand increased the side pressure above the water pressure, engineers would expect to use more compressed air to hold it back, while, as a matter of fact, the air pressure used seldom varies much from that called for by the hydrostatic head.

Although allowance for water pressure is sufficient for designing retaining walls in New York quicksand, it is far from sufficient in certain silty materials. For instance, in Maryland, a coffer-dam, excavated to a depth of 30 ft. in silt and water, had the bottom shoved in 2 ft., in spite of the fact that the waling pieces were 5 ft. apart vertically at the top and 3 ft. at the bottom, and were braced with 12 by 12-in. timbers, every 7 ft. horizontally. The walings split, and the cross-braces cut into the waling pieces from 1 to 2 in.; in other words, the pressure seemed to be almost irresistible. This is quite a contrast to certain excavations in Brooklyn, which, without any bracing whatever, were safely carried down 15 ft.

Any engineer who tries to guess at the angle of repose, and, from the resulting calculations, economizes on his bottom struts, will find that sooner or later an accident on one job will cause enough loss of life and money to pay for conservative timbers for the rest of his life. So much for side pressures. As to the pressure in the roof of a tunnel, probably every engineer will agree that almost any material except

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unfrozen water will tend to arch more or less, but how much it is impossible to say. It is doubtful whether any experienced engineer would ever try to carry all the weight over the roof, except in the case of back-fill, and even then he would have to make his own assumption (which sounds more polite than "guess"). Mr. Thomson.

The author has stated, however, that when the tunnel roof and sides are in place, no further trouble need be feared. On the contrary, in 1885, the Canadian Pacific Railroad built a tunnel through clayey material and lined it with ordinary 12 by 12-in. timber framing, about 2 or 3 ft. apart. After the tunnel was completed, it collapsed. It was re-excavated and lined with 12 by 12-in. timbers side by side, and it collapsed again; then the tunnel was abandoned, and, for some 20 years, the track, carried around on a 23° curve, was used until a new tunnel was built farther in. This trouble could have been caused either by the sliding or swelling of the material, and the speaker is inclined to believe that it was caused by swelling, for it is known, of course, that most material has been deposited by Nature under great pressure, and, by excavating in certain materials, the air and moisture would cause those materials to swell and become an irresistible force.

To carry the load, Mr. Meem prefers to rely on the points of the piles rather than the side friction. In such cases the pile would act as a post, and would probably fail when ordinarily loaded, unless firmly supported at the sides. The speaker has seen piles driven from 80 to 90 ft. in 10 min., which offered almost no resistance, and yet, a few days later, they would sustain 40 tons each. No one would dream of putting 40 tons on a 90-ft. pile resting on rock, if it were not adequately supported.

It is the speaker's opinion that bracing should not be omitted for either piles or coffer-dams.

Charles E. Gregory, Assoc. M. Am. Soc. C. E.—In describing his last experiment with the hydraulic chambers and plunger, Mr. Meem states that, after letting the pressure stand at 25 lb., etc., the piston came up. This suggests that the piston might have been raised at a much lower pressure, if it had been allowed to stand long enough. Mr. Gregory.

The depth and coarseness of the sand were not varied to ascertain whether any relation exists between them and the pressure required to lift the piston. If the pressure varied with the depth of sand, it would indicate that the reduction was due to the resistance of the water when finely divided by the sand; if it varied with the coarseness of the sand, as it undoubtedly would, especially if the sand grains were increased to spheres 1 in. in diameter, it would show that it was independent of the voids in the sand, but dependent on dividing the water into thin films.

The speaker believes that the greater part of the reduction of pressure on the bottom of the piston might be better explained by

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Mr. Gregory. the viscosity of the water, than to assume that a considerable part of the plunger is not in contact with it. The water, being divided by fine sand into very thin films, has a tensile strength which is capable of resisting the pressure for at least a limited time.

If the water is capable of exerting its full hydrostatic pressure through the sand, the total pressure would be the full hydrostatic pressure on the bottom of the piston where in contact, and, where separated from it by a grain of sand, the pressure would be decreased only by the weight of the grain. If a large proportion of the top area of a grain is in contact, as assumed by the author, this reduction of pressure would be very small. A correct interpretation can be obtained only after more complete experiments have been made.

For horizontal pressures exerted by saturated sands on vertical walls, it has not been demonstrated that anything should be deducted from full water pressure. No matter how much of the area is in direct contact with the sand rather than the water, the full water pressure would be transmitted through each sand grain from its other side and, if necessary, from and through many other grains which may be in turn in contact with it. The pressure on such a wall will be water pressure over its entire surface, and, in addition, the thrust of the sand after correcting for its loss of weight in the water.

The fact that small cavities may be excavated from the sides of trenches or tunnels back of the sheeting proves only that there is a local temporary arching of the material, or that the cohesion of the particles is sufficient to withstand the stress temporarily, or that there is a combination of cohesion and arching. The possibility of making such excavations does not prove that pressure does not exist at such points. That sand or earth will arch under certain conditions has long been an accepted fact. The sand arches experimented with developed their strength only after considerable yielding and, therefore, give no index of the distribution or intensity of stress before such yielding. Furthermore, sand and earth in Nature are not constrained by forms and reinforcing rods.

Mr. Meem's paper is very valuable in that it presents some unusual phenomena, but many of the conclusions drawn therefrom cannot be accepted without further demonstration.

Mr. Perry. FRANCIS W. PERRY, ASSOC. M. AM. SOC. C. E.—Pressure-gauge observations on a number of pneumatic caissons recently sunk, through various grades of sand, to rock at depths of from 85 to 105 ft. below ground-water, invariably showed working-chamber air-pressures equal, as closely as could be observed, to the hydrostatic pressures computed, for corresponding depths of cutting-edge, as given in Table 2.

These observations and computations were made by the speaker in connection with the caisson foundations for the Municipal Building, New York City.

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Mr. Perry.

TABLE 2.—Equivalent Feet of Depth Below Water Per Pound Pressure.

Pressure, in pounds.	Equivalent feet of depth. M. H. W.	Equivalent elevation for water at — 6.85. Ground-water.	Observed pressure.	Pressure, in pounds.	Equivalent feet of depth. M. H. W.	Equivalent elevation for water at — 6.85. Ground-water.	Observed pressure.
1	2.31	9.06	Practically the same as computed for ground-water.	24	55.51	62.36	Practically the same as computed for ground-water.
2	4.63	11.48		25	57.82	64.67	
3	6.94	13.79		26	60.14	66.99	
4	9.25	16.10		27	62.45	69.30	
5	11.57	18.42		28	64.76	71.61	
6	13.88	20.73		29	67.08	73.93	
7	16.19	23.04		30	69.39	76.24	
8	18.50	25.35		31	71.70	78.55	
9	20.82	27.67		32	74.01	80.86	
10	23.13	29.98		33	76.33	83.18	
11	25.44	32.29		34	78.64	85.49	
12	27.76	34.61		35	80.95	87.80	
13	30.07	36.92		36	83.27	90.12	
14	32.38	39.23		37	85.58	92.43	
15	34.70	41.55		38	87.89	94.74	
16	37.01	43.86		39	90.20	97.05	
17	39.32	46.17		40	92.52	99.37	
18	41.63	48.48		41	94.83	101.68	
19	43.95	50.80		42	97.14	103.99	
20	46.26	53.11		43	99.46	106.31	
21	48.57	55.42		44	101.77	108.62	
22	50.89	57.74		45	104.08	110.93	
23	53.20	60.05		46	106.39	113.24	

Note.—Equivalent depth in feet $= \frac{34}{14.7} \times$ pressure.

E. P. Goodrich, M. Am. Soc. C. E. (by letter).—This paper is to be characterized by superlatives. Parts of it are believed to be exceptionally good, while other parts are considered equally dangerous. The author's experimental work is extremely interesting, and the writer believes the results obtained to be of great value; but the analytical work, both mathematical and logical, is emphatically questioned. Mr. Goodrich.

The writer believes that, in the design of permanent structures, consideration of arch action should not be included, at least, not until much more information has been obtained. He also believes that the design of temporary structures with this inclusion is actually dangerous in some instances, and takes the liberty of citing the following statement by the author, with regard to his first experiment:

"About an hour after the superimposed load had been removed, the writer jostled the box with his foot sufficiently to dislodge some of the exposed sand, when the arch at once collapsed and the bottom fell to the ground."

The writer emphatically questions the author's ideas as to "the thickness of key" which "should be allowed" over tunnels, believing that conditions within an earth mass, except in very rare instances, are

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Mr. Goodrich. such that true arch action will seldom take place to any definite extent, through any considerable depths. Furthermore, the author's reason for bisecting the angle between the vertical and the angle of repose of the material, when he undertakes to determine the thickness of key, is not obvious. This assumption is shown to be absurd when carried to either limit, for when the angle of repose equals zero, as is the case with water, this method would give a definite thickness of key, while there can be absolutely no arch action possible in such a case; and, when the angle of repose is 90°, as may be assumed in the case of rock, this method would give an infinite thickness of key, which is again seen to be absurd. It would seem as if altogether too many unknowable conditions had been assumed. In any case, no arch action can be brought into play until a certain amount of settlement has taken place so as to bring the particles into closer contact, and in such a way that the internal stresses are practically those only of compression, and the shearing stresses are within the limits possible for the material in question.

The author has repeatedly made assumptions which are not borne out by the application of his mathematical formulas to actual extreme conditions. This method of application to limiting conditions is concededly sometimes faulty; but the writer believes that no earth pressure theory, or one concerning arch action, can be considered as satisfactory which does not apply equally well to hydraulic pressure problems when the proper assumptions are made as to the factors for friction, cohesion, etc. For example, when the angle of repose is considered as zero, in the author's first formula for W_1, the value becomes $\frac{1}{2}$ W_1, whereas it should depend solely on the depth, which does not enter the formula, and not at all on the width of opening, l, which is thus included.

The author has given no experiments to prove his statement that "the arch thrust is greater in dryer sand," and the accuracy of the statement is questioned. Again, no reason is apparent for assuming the direction of the "rakers" in Fig. 3 as that of the angle of repose. The writer cannot see why that particular angle is repeatedly used, when almost any other would give results of a similar kind. The author has made no experiments which show any connection between the angle of repose, as he interprets it, and the lines of arch action which he assumes to exist.

With regard to the illustration of the condition which is thought to exist when the "material is composed of large bowling balls," supposedly all of the same size, the writer believes the conclusion to be erroneous, and that this can be readily seen by inspection of a diagram in which such balls are represented as forming a pile similar to the well-known "pile of shells" of the algebras, in the diagram of which a pile of three shells, resting on the base, has been omitted. It is then seen that unless the pressures at an angle of 60° with the horizontal

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Mr. Goodrich.

are sufficient to produce frictional resistance of a very large amount, the balls will roll and instantly break the arch action suggested by the author. Consequently, an almost infinitesimal settlement of the "centering" may cause the complete destruction of an arch of earth.

The author's logic is believed to be entirely faulty in many cases because he repeatedly makes assumptions which are not in accordance with demonstrated fact, and finally sums up the results by the statement: "It is conceded" (line 2, p. 357, for example), when the writer, for one, has not even conceded the accuracy of the assumptions. For instance, the author's well-known theory that pressures against retaining walls are a maximum at the top and decrease to zero at the bottom, is in absolute contradiction to the results of experiments conducted on a large scale by the writer on the new reinforced concrete retaining wall near the St. George Ferry, on Staten Island, New York City, which will soon be published, and in which the usual law of increase of lateral pressure with depth is believed to be demonstrated beyond question. It must be conceded that a considerable arch action (so-called) actually exists in many cases; but it should be equally conceded by the advocates of the existence of such action that changes in humidity, due to moving water, vibration, and appreciable viscosity, etc., will invariably destroy this action in time. In consequence, the author's reasoning in regard to the pressures against the faces of retaining walls is believed to be open to grave question as to accuracy of assumption, method, and conclusion.

The author is correct in so far as he assumes that "the character of the stresses due to the thrust of the material will" not "change if bracing should be substituted for the material in the area" designated by him, etc., provided he makes the further assumption that absolutely no motion, however infinitesimal, has taken place meantime; but, unless such motion has actually taken place, no arch action can have developed. An arch thrust can result only with true arch action, that is, with stable abutments, and the mass stressed wholly in compression, with corresponding shortening of the arch line. The arch thrust must be proportional to the elastic deformation (shortening) of the arch line. If any such arch as is shown in Fig. 5 is assumed to carry the whole of the weight of material above it, that assumed arch must relieve all the assumed arches below. Therefore each of the assumed arches can carry nothing more than its own mass. Otherwise the resulting thrust would increase with the depth, which is opposed to the author's theory.

Turning again to the condition that each arch can carry only its own weight: if these arches are assumed of thicknesses proportional to the distance upward from the bottom of the wall, they will be similar figures, and it is easily demonstrated that the thrust will then be uniform in amount throughout the whole height of the wall, except,

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Mr. Goodrich. perhaps, at the very top. This condition is contrary to the author's ideas and also to the facts as demonstrated by the writer's experiment on the 40-ft. retaining wall at St. George. Consequently, the author's statement: "nor can anyone * * * doubt that the top timbers are stressed more heavily than those at the bottom," is emphatically doubted and earnestly denied by the writer. Furthermore, "the assumption" made by the author as to "the tendency of the material to slide" so as to cause it "to wedge * * * between the face of the sheeting * * * and some plane between the sheeting and the plane of repose," is considered as absolutely unwarranted, and consequently the whole conclusion is believed to be unjustified. Nor is the author's assumption (line 5, p. 361), that "the thrust * * * is measured by its weight divided by the tangent of the * * * angle of repose" at all obvious.

The author presents some very interesting photographs showing the natural surface slopes of various materials; but it is interesting to note that he describes these slopes as having been produced by the "continual slipping down of particles." The vast difference between angles of repose produced in this manner by the rolling friction of particles and the internal angles of friction, which must be used in all earth-pressure investigations, has been repeatedly called to the attention of engineers by the writer.*

The writer's experiments are entirely in accord with those of the author in which the latter claims to demonstrate that "earth and water pressures act independently of each other," and the writer is much delighted that his own experiments have been thus confirmed.

In Experiment No. 3, the query is naturally suggested: "What would have been the result if the nuts and washers had first been tightened and water then added?" Although the writer has not tried the experiment, he is rather inclined to the idea that the arch would have collapsed. With regard to Experiment No. 5, there is to be noted an interesting possibility of its application to the theoretical discussion of masonry dams, in which films of water are assumed to exist beneath the structure or in crevices or cracks of capillary dimensions. The writer has always considered the assumptions made by many designing engineers as unnecessarily conservative. In regard to the author's conclusions from Experiment No. 6, it should be noted that no friction can exist between particles of sand and surrounding water unless there is a tendency of the latter to move; and that water in motion does not exert pressures equal to those produced when in a static condition, the reduction being proportional to the velocity of flow.

The author's conclusion (p. 371), that "pressure will caues the quicksand to set up hydraulic action," does not seem to have been

* "Lateral Earth Pressures and Related Phenomena," *Transactions*, Am. Soc. C. E., Vol. LIII, p. 272.

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demonstrated by his experiments, but to be only his theory. In this instance, the results of the writer's experiments are contrary to the author's theory and conclusion. Mr. Goodrich.

The writer will heartily add his protest to that of the author "against considering semi-aqueous masses, such as soupy sands, soft concrete, etc., as exerting hydrostatic pressure due to their weight in bulk, instead of to the specific gravity of the basic liquid." Again, similarly hearty concurrence is given to the author's statement:

"If the solid material in any liquid is agitated, so that it is virtually in suspension, it cannot add to the pressure, and if allowed to subside it acts as a solid, independently of the water contained with it, although the water may change somewhat the properties of the material, by increasing or changing its cohesion, angle of repose, etc."

On the other hand, it is believed that the author's statement, as to "the tendency of marbles to arch," a few lines above the one last quoted, should be qualified by the addition of the words, "only when a certain amount of deflection has taken place so as to bring the arch into action." Again, on the following page, a somewhat similar qualification should be added to the sentence referring to the soft clay arch, that it would "stand if the rods supporting the intrados of the arch were keyed back to washers covering a sufficiently large area," by inserting the words, "unless creeping pressures (such as those encountered by the writer in his experiments) were exceeded."

The writer considers as very doubtful the formula for D_x, which is the same as that for W_1, already discussed. The author's statement that "additional back-fill will [under certain circumstances] lighten the load on the structure," is considered subject to modification by some such clause as the following, "the word 'lighten' here being understood to mean the reduction to some extent of what would be the total pressure due to the combined original and added back-fill, provided no arch action occurred."

The writer is in entire agreement with the author as to the probability that water is often "cut off absolutely from its source of pressure," with the attendant results described by the author (p. 378); and again, that too little attention has been given to the bearing power of soil, with the author's accompanying criticism.

The writer cannot see, however, where the author's experiments demonstrate his statement "that pressure is transmitted laterally through ground, most probably along or nearly parallel to the angles of repose," or any of the conclusions drawn by him in the paragraph (p. 381), which contains this questionable statement. Again the writer is at a loss as to how to interpret the statement that the author has found that "better resistance" has been offered by "small open caissons sunk to a depth of a few feet and cleaned out and filled with concrete" than by "spreading the foundation over four or five times the equiva-

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Mr. Goodrich. lent area." The writer agrees with the author in the majority of his statements as to the "bearing value and friction on piles," but believes that he is indulging in pure theory in some of his succeeding remarks, wherein he ascribes to arch action the results which he believes would be observed if "a long shaft be withdrawn vertically from moulding sand." These phenomena would be due rather to capillary action and the resulting cohesion.

Naturally, the writer doubts the author's conclusions as to the pressure at the top of large square caisson shafts when he states that "the pressure at the top * * * will * * * increase proportionately to the depth." Again, the author is apparently not conversant with experiments made by the Dock Department of New York City, concerning piles driven in the Hudson River silt, which showed that a single heavily loaded pile carried downward with it other unloaded piles, driven considerable distances away, showing that it was not the pile which lacked in resistance, as much as the surrounding earth.

In conclusion, the writer heartily concurs with the statement that "too much has been taken for granted in connection with earth pressures and resistance," and he is sorry to be forced to add that he believes the author to be open to the criticism which he himself suggests, that "both in experimenting and observing, the engineer [and in this case the author] will frequently find what is being looked for or expected and will fail to see the obvious alternative."

Mr. Pruyn. FRANCIS L. PRUYN, M. AM. SOC. C. E. (by letter).—Mr. Meem should be congratulated, both in regard to the highly interesting theories which he advances on the subject of sand pressures—the pressures of subaqueous material—and on his interesting experiments in connection therewith.

The experiment in which the plunger on the hydraulic ram is immersed in sand and covered with water does not seem to be conclusive. By this experiment the author attempts to demonstrate that the pressure of the water transmitted through the sand is only about 40% as great as when the sand is not there. The travel of ground-water through the earth is at times very slow, and occasionally only at the rate of from 2 to 3 ft. per hour. In the writer's opinion, Mr. Meem's experiment did not cover sufficient time during which the pressure was maintained at any given point. It is quite probable that it may take 15 or 20 min. for the full pressure to be transmitted through the sand to the bottom of the plunger, and it is hoped, therefore, that he will make further experiments lasting long enough to demonstrate this point.

In regard to the question of skin friction on caissons and piles, it may be of interest to mention an experiment which the writer made during the sinking of the large caissons for the Williamsburg Bridge.

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These caissons were about 70 ft. long and 50 ft. wide. The river bottom was about 50 ft. below mean high water, and the caissons penetrated sand of good quality to a depth of from 90 to 100 ft. below that level. On two occasions calculations were made to determine the skin friction while the caissons were being settled. With the cutting edge from 20 to 30 ft. below the river bottom, the calculations showed that the skin friction was between 500 and 600 lb. per sq. ft. The writer agrees with Mr. Meem that, in the sinking of caissons, the arch action of sand is, in a great measure, destroyed by the compressed air which escapes under the cutting edge and percolates up through the material close to the sides of the caissons. Mr. Pruyn.

With reference to the skin friction on piles, the writer agrees with Mr. Meem that in certain classes of material this is almost a negligible quantity. The writer has jacked down 9-in. pipes in various parts of New York City, and by placing a recording gauge on the hydraulic jack, the skin friction on the pile could be obtained very accurately. In several instances the gauge readings did not vary materially from the surface down to a penetration of 50 ft. In these instances the material inside the pipe was cleaned out to within 1 ft. of the bottom of the pile, so that the gauge reading indicated only the friction on the outside of the pipe plus the bearing value developed by its lower edge. For a 9-in. pipe, the skin friction on the pile plus the bearing area of the bottom of the pipe seems to be about 20 tons, irrespective of the depth. After the pipe had reached sufficient depth, it was concreted, and, after the concrete had set, the jack was again placed on it and gauge readings were taken. It was found that in ordinary sands the concreted steel pile would go down from 3 to 6 in., after which it would bring up to the full capacity of a 60-ton jack, showing, by gauge reading, a reaction of from 70 to 80 tons.

It is the writer's opinion that, in reasonably compact sands situated at a depth below the surface which will not allow of much lateral movement, a reaction of 100 tons per sq. ft. of area can be obtained without any difficulty whatever.

Frank H. Carter, Assoc. M. Am. Soc. C. E. (by letter).—Mr. Meem has contributed much that is of value, particularly on water pressures in sand; just what result would be obtained if coarse crushed stone or similar material were substituted for sand in Experiment No. 6, is not obvious. Mr. Carter.

It has been the practice lately, among some engineers in Boston, as well as in New York City, to assume that water pressures on the underside of inverts is exerted on one-half the area only. The writer, however, has made it a practice first to lay a few inches of cracked stone on the bottom of wet excavations in order to keep water from concrete which is to be placed in the invert. In addition to the

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Mr. Carter. cracked stone under the inverts, shallow trenches dug laterally across the excavation to insure more perfect drainage, have been observed. Both these factors no doubt assist the free course of water in exerting pressure on the finished invert after the underdrains have been closed up on completion of the work. The writer, therefore, awaits with interest the repetition of Experiment No. 6, with water on the bottom of a piston buried in coarse gravel or cracked stone.

As for the arching effect of sand, the writer believes that Mr. Meem has demonstrated an important principle, on a small scale. It must be regretted, however, that the box was not made larger, for, to the writer, it appears unsafe to draw such sweeping conclusions from small experiments. As small models of sailboats fail to develop completely laws for the design and control of large racing yachts, so experiments in small sand boxes may fail to demonstrate the laws governing actual pressures on full-sized structures.

For some time the writer has been using a process of reasoning similar to that of the author for assumptions of earth pressure on the roofs of tunnel arches, except that the vertical forces assumed to hold up the weight of the earth have been ascribed to cohesion and friction, along what might be termed the sides of the "trench excavation."

The writer fails to find proof in this paper of the author's statement that earth pressures on the sides of a structure buried in earth are greater at the top than at the bottom of a trench. That some banks are "top-heavy," is, no doubt, a fact, the writer having often heard similar expressions used by experienced trench foremen, but, in every case called to his attention, local circumstances have caused the top-heaviness, either undermining at the bottom of the trench, too much banked earth on top, or the earth excavated from the trench being too near the edge of the cut.

For some years the writer has been making extended observations on deep trenches, and, thus far, has failed to find evidence, except in aqueous material, of earth pressures which might be expected from the known natural slope of the material after exposure to the elements; and this latter feature may explain why sheeted trenches stand so much better than expected. If air had free access to the material, cohesion would be destroyed, and theoretical pressures would be more easily developed. With closely-sheeted trenches, weathering is practically excluded, and the bracing, which seemingly is far too light, holds up the trench with scarcely a mark of pressure. As an instance, in 1893, the writer was successfully digging sewer trenches from 10 to 14 ft. deep, through gravel, in the central part of Connecticut, without bracing; because of demands of the work in another part of the city, a length of several hundred feet of trench was left open for three days, resulting in the caving-in of the sides. The elements had destroyed the cohesion, and the sides of the trenches no longer stood vertically.

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Recently, in the vicinity of Boston, trenches, 32 ft. wide, and from 25 to 35 ft. deep, with heavy buildings on one side, have been braced with 8 by 10-in. stringers, and bracers at 10-ft. centers longitudinally, and from 3 to 5 ft. apart vertically; this timbering apparently was too slight for pressures which, theoretically, might be expected from the natural slope of the material. Just what pressures develop on the sides of the structures in these deep trenches after pulling the top sheeting (the bottom sheeting being left in place) is, of course, a matter of conjecture. There can be no doubt that there is an arching of the material, as suggested by the author. How much this may be assisted by the practical non-disturbance of the virgin material is, of course, indeterminate. That substructures and retaining walls designed according to the Rankine or similar theories have an additional factor of safety from too generous an assumption in regard to earth pressure is practically admitted everywhere. It is almost an engineering axiom that retaining walls generally fail because of insufficient foundation only. Mr. Carter.

For the foregoing reasons, and particularly from observations on the effect of earth pressures on wooden timbers used as bracing, the writer believes that, ordinarily, the theoretical earth pressures computed by Rankine and Coulomb are not realized by one-half, and sometimes not even by one-third or one-quarter in trenches well underdrained, rapidly excavated, and thoroughly braced.

J. C. Meem, M. Am. Soc. C. E. (by letter).—The writer has been much interested in this discussion, and believes that it will be of general value to the profession. It is unfortunate, however, that several of the points raised have been due to a careless reading of, or failure to understand, the paper. Mr. Meem.

Taking up the discussion in detail, the writer will first answer the criticisms of Mr. Goodrich. He says:

"The writer believes that, in the design of permanent structures, consideration of arch action should not be included, at least, not until more information has been obtained. He also believes that the design of temporary structures with this inclusion is actually dangerous in some instances."

If the arching action of earth exists, why should it not be recognized and considered? The design of timbering for a structure to rest, for instance, at a depth of from 200 to 300 ft. in normal dry earth, without considering this action, would be virtually prohibitive.

Mr. Goodrich proceeds to show one of the dangers of considering such action by quoting the writer, as follows:

"About an hour after the superimposed load had been removed, the writer jostled the box with his foot sufficiently to dislodge some of the exposed sand, when the arch at once collapsed and the bottom fell to the ground."

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Mr. Meem.

He fails, as do so many other critics of this theory, to distinguish the difference between that portion of the sand which acts as so-called "centering" and that which goes to make up the sustaining arch. The dislodgment of any large portion of this "centering" naturally causes collapse, unless it is caught, in which case the void in the "centering" is filled from the material in the sustaining arch, and this, in turn, is filled from that above, and so on, until the stability of each arch is in turn finally established. This, however, does not mean that, during the process of establishing this equilibrium of the arch stresses, there is no arching action of any of the material above, but only that some of the so-called arches are temporarily sustained by those below. That is, in effect, each area of the material above becomes, in turn, a dependent, an independent, and finally an interdependent arch.

If Mr. Goodrich's experience has led him to examine any large number of tunnel arches or brick sewers, he will have noted in many of them longitudinal cracks at the soffits of the arches and perhaps elsewhere. These result from three causes:

First.—In tunneling, there is more or less loss of material, while, in back-filling, the material does not at first reach its final compactness. Therefore, in adjusting itself to normal conditions, this material causes impact loads to come upon the green arch, and these tend to crack it.

Second.—No matter how tightly a brick or other arch is keyed in, there must always be some slight subsidence when the "centers" are struck. This, again, results in a shock, or impact loading, to the detriment of the arch.

Third.—The most prolific cause, however, is that in tunneling, as well as in back-filling open cuts, the material backing up the haunches is more or less loosened and therefore is not at first compact enough to prevent the spreading of the haunches when the load comes on the arch. This causes cracking, but, as soon as the haunches have been pressed out against the solid material, the cracking usually ceases, unless the pressure has been sufficiently heavy to cause collapse.

An interesting example of this was noted in the Joralemon Street branch of the Rapid Transit Tunnel, in Brooklyn, in which a great many of the cast-iron rings were cracked under the crown of the arch, during construction; but, in spite of this, they sustained, for more than two years, a loading which, according to Mr. Goodrich, was continually increasing. In other words, the cracked arch sustained a greater loading than that which cracked the plates during construction, according to his theory, as noted in the following quotation:

"But it should be equally conceded by the advocates of the existence of such action that changes in humidity, due to moving water, vibra-

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tion, and appreciable viscosity, etc., will invariably destroy this action in time." Mr. Meem.

As to the correctness of this theory Mr. Goodrich would probably have great difficulty in convincing naturalists, who are aware that many animals live in enlarged burrows the stability of which is dependent on the arching action of the earth; in fact, many of these burrows have entrances under water. He would also have some difficulty in convincing those experienced miners who, after a cave-in, always wait until the ground has settled and compacted itself before tunneling, usually with apparent safety, over the scene of the cave-in.

The writer quotes as follows from Mr. Goodrich's discussion:

"In any case, no arch action can be brought into play until a certain amount of settlement has taken place so as to bring the particles into closer contact, and in such a way that the internal stresses are practically those only of compression, and the shearing stresses are within the limits possible for the material in question."

Further:

"Consequently, an almost infinitesimal settlement of the 'centering' may cause the complete destruction of an arch of earth."

And further:

"On the other hand, it is believed that the author's statement, as to the 'tendency of marbles to arch,' * * * should be qualified by the addition of the words, 'only when a certain amount of deflection has taken place so as to bring the arch into action.'"

In a large measure the writer agrees with the first and last quotations, but sees no reason to endorse the second, as it is impossible to consider any arch being built which does not settle slightly, at least, when the "centers" are struck.

Regarding his criticism of the lack of arching action in balls or marbles, he seems to reason that the movement of the marbles would destroy the arch action. It is very difficult for the writer to conceive how it would be possible for balls or marbles to move when confined as they would be confined if the earth were composed of them instead of its present ingredients, and under the same conditions otherwise. Mr. Goodrich can demonstrate the correctness of the writer's theories, however, if he will repeat the writer's Experiment No. 3, with marbles, with buckshot, and with dry sand. He is also advised to make the experiment with sand and water, described by the writer, and is assured that, if he will see that the washers are absolutely tight before putting the water into the box, he can do this without bringing about the collapse of the arch; the only essential condition is that the bottom shall be keyed up tightly, so as not to allow the escape of any sand. He is also referred to the two photographs, Plate XXIV, illustrating the writer's first experiment, showing how increases in the loading

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Mr. Meem. resulted in compacting the material of the arch and in the consequent lowering of the false bottom. As long as the exposed sand above this false bottom had cohesion enough to prevent the collapse of the "centering," this arch could have been loaded with safety up to the limits of the compressive strength of the sand.

To quote again from Mr. Goodrich:

"Furthermore, the author's reason for bisecting the angle between the vertical and the angle of repose of the material, when he undertakes to determine the thickness of key, is not obvious. This assumption is shown to be absurd when carried to either limit, for when the angle of repose equals zero, as is the case with water, this method would give a definite thickness of key, while there can be absolutely no arch action possible in such a case; and, when the angle of repose is 90°, as may be assumed in the case of rock, this method would give an infinite thickness of key, which is again seen to be absurd."

Mr. Goodrich assumes that water or liquid has an angle of repose equal to zero, which is true, but the writer's assumptions applied only to solid material, and the liquid gives an essentially different condition of pressure, as shown by a careful reading of the paper. In solid rock Mr. Goodrich assumes an angle of repose equal to 90°, for which there is no authority; that is, solid rock has no known angle of repose. In order to carry these assumptions to a definite conclusion, we must assume for that material with an angle of repose of 90° some solid material which has weight but no thrust, such as blocks of ice piled vertically. In this case Mr. Goodrich can readily see that there will be no arching action over the structure, and that the required thickness of key would be infinite. As to the other case, it is somewhat difficult to conceive of a solid with an angle of repose of zero; aqueous material does not fulfill this condition, as it is either a liquid or a combination of water and solid material. The best illustration, perhaps, would be to assume a material composed of iron filings, into which had been driven a powerful magnet, so that the iron filings would be drawn horizontally in one direction. It is easy to conceive, then, that in tunneling through this material there would be no necessity for holding up the roof; the definite thickness of key given, as being at the point of intersection of two 45° angles, would be merely a precautionary measure, and would not be required in practice.

It is thus seen that both these conditions can be fulfilled with practical illustrations; that is, for an angle of repose of 90°, that material which has weight and no thrust, and for an angle of repose of zero, that solid material which has thrust but no weight.

Mr. Goodrich says the author has given no experiments to prove his statement that the arch thrust is greater in dryer sand. If Mr. Goodrich will make the experiment partially described as Experiment No. 3, with absolutely dry sand, and with moist sand, and on a scale

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large enough to eliminate cohesion, he will probably find enough to convince him that in this assumption the writer is correct. At the same time, the writer has based his theory in this regard on facts which are not entirely conclusive, and his mind is open as to what future experiments on a large scale may develop. It is very probable, however, that an analytical and practical examination of the English experiments noted on pages 379 and 380, will be sufficient to develop this fact conclusively. Mr. Meem.

The writer is forced to conclude that some of the criticisms by Mr. Goodrich result from a not too careful reading of the paper. For instance, he states:

" 'It is conceded' (line 2, p. 357, for example) when the writer, for one, has not even conceded the accuracy of the assumptions."

A more careful reading would have shown Mr. Goodrich that this concession was one of the writer's as to certain pressures against or on tunnels, and, if Mr. Goodrich does not concede this, he is even more radical than the writer.

And again:

" 'Nor can anyone * * * doubt that the top timbers are stressed more heavily than those at the bottom' is emphatically doubted and earnestly denied by the writer."

It is unfortunate that Mr. Goodrich failed to make the complete quotation, which reads:

"Nor can anyone, looking at Fig. 5, doubt," etc.

A glance at Fig. 5 will demonstrate that, under conditions there set forth, the writer is probably correct in his assertion as relating to that particular instance. Further:

"For instance, the author's well-known theory that the pressures against retaining walls are a maximum at the top and decrease to zero at the bottom, is in absolute contradiction to the results of experiments conducted on a large scale by the writer on the new reinforced concrete retaining wall near the St. George Ferry, on Staten Island."

The writer's "well-known theory that pressures against retaining walls are a maximum at the top and decrease to zero at the bottom" applies only to pressures exerted by absolutely dry and normally dry material, and it seems to him that this so-called theory is capable of such easy demonstration, by the simple observation of any bracing in a deep trench in material of this class, that it ought to be accepted as at least safer than the old theory which it reverses. As to this "well-known theory" in material subject to water pressure, a careful reading of the paper, or an examination of Fig. 12 and its accompanying text, or an examination of Table 1, will convince Mr. Goodrich that, under the writer's analysis, this pressure does not decrease to

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Mr. Meem. zero at the bottom, but that in soft materials it may be approximately constant all the way down, while, in exceptionally soft material, conditions may arise where it may increase toward the bottom. The determination should be made by taking the solid material and drying it sufficiently so that water does not flow or seep from it. When this material is then compacted to the condition in which it would be in its natural state, its angle of repose may be measured, and may be found to be as high as 60 degrees. The very fine matter should then be separated from the coarser material, and the latter weighed, to determine its proportion. Subtracting this from the total, the remainder could be credited to "aqueous matter." It is thus seen that with a material when partially dried in which the natural angle of repose might be 60°, and in which the percentage of water or aqueous matter when submerged might be 60%, there would be an increase of pressure toward the bottom.

The writer does not know the exact nature of the experiments made at St. George's Ferry by Mr. Goodrich, but he supposes they were measurements of pressures on pistons through holes in the sheeting. He desires to state again that he cannot regard such experiments as conclusive, and believes that they are of comparative value only, as such experiments do not measure in any large degree the pressure of the solid material but only all or a portion of the so-called aqueous matter, that is, the liquid and very fine material which flows with it. Thus it is well known that, during the construction of the recent Hudson and North River Tunnels, pressures were tested in the silt, some of which showed that the silt exerted full hydrostatic pressure. At the same time, W. I. Aims, M. Am. Soc. C. E., stated in a public lecture, and recently also to the writer, that in 1890 he made some tests of the pressure of this silt in normal air for the late W. R. Hutton, M. Am. Soc. C. E. A hole, 12 in. square, was cut through the brickwork and the iron lining, just back of the lock in the north tube (in normal air), and about 1 000 ft. from the New Jersey shore. It was found that the silt had become so firm that it did not flow into the opening. Later, a 4-in. collar and piston were built into the opening, and, during a period covering at least 3 months, constant observations showed that no pressure came upon it; in fact, it was stated that the piston was frequently worked back and forth to induce pressure, but no response was obtained during all this period. The conclusion must then be drawn that when construction, with its attendant disturbance, has stopped, the solid material surrounding structures tends to compact itself more or less, and solidify, according as it is more or less porous, forming in many instances what may be virtually a compact arch shutting off a large percentage of the normal, and some percentage even of the aqueous, pressure.

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Mr. Meem.

That the pressure of normally dry material cannot be measured through small openings can be verified by any one who will examine such material back of bracing showing evidences of heavy pressure. The investigator will find that, if this material is free from water pressure, paper stuffed lightly into small openings will hold back indefinitely material which in large masses has frequently caused bracing to buckle and sheeting planks to bend and break; and the writer reiterates that such experiments should be made in trenches sheeted with horizontal sheeting bearing against short vertical rangers and braces giving horizontal sections absolutely detached and independent of each other. In no other way can such experiments be of real value (and even then only when made on a large scale) to determine conclusively the pressure of earth on trenches.

As to the questions of the relative thrust of materials under various angles of repose, and of the necessity of dividing by the tangent, etc.; these, to the writer, seem to be merely the solution of problems in simple graphics.

The writer believes that if Mr. Goodrich will make, even on a small scale, some of the experiments noted by the writer, he will be convinced that many of the assumptions which he cannot at present endorse are based on fact, and his co-operation will be welcomed with the greatest interest. Among the experiments which he is asked to make is the one in dry sand, noted as Experiment No. 3, whereby it can be shown very conclusively that additional back-fill will result in increased arching stability, on an arch which would collapse under lighter loading.

The writer is indebted to Mr. Goodrich for pointing out some errors in omission and in typography (now corrected), and for his hearty concurrence in some of the assumptions which the writer believed would meet with greatest disapproval.

In reply to Mr. Pruyn and Mr. Gregory, the writer assumed that the piston area in Experiment No. 6 should be reduced only by the actual contact of material with it. If this material in contact should be composed of theoretical spheres, resulting in a contact with points only, then the theoretical area reduced should be in proportion to this amount only. The writer does not believe, however, that this condition exists in practice, but thinks that the area is reduced very much more than by the actual theoretical contact of the material. He sees no reason, as far as he has gone, to doubt the accuracy of the deductions from this experiment.

Regarding the question of the length of time required to raise the piston, he does not believe that the position of his critics is entirely correct in this matter; that is, it must either be conceded that the piston area is cut off from the source of pressure, or that it is in

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Mr. Meem. contact with it through more or less minute channels of water. If it is cut off, then the writer's contention is proved without the need of the experiment, and it is therefore conclusive that a submerged tunnel is not under aqueous pressure or the buoyant action of water. If, on the other hand, the water is in contact through channels bearing directly upon the piston and leading to the clear water chamber, any increase in pressure in the water chamber must necessarily result in a virtually instantaneous increase of the pressure against the piston, and therefore the action on the latter should follow almost immediately. In all cases during the experiments the piston did not respond until the pressure was approximately twice as great as required in clear water, therefore the writer must conclude either that the experiments proved it conclusively or that his assumption is proved without the necessity of the experiments. That is, the pressure is virtually not in evidence until the piston has commenced to move.

Mr. Pruyn has added valuable information in his presentation of data obtained from specific tests of the bearing value of, and friction on, hollow steel piles. These data largely corroborate tests and observations by the writer, and are commended to general attention.

Mr. Carter's information is also of special interest to the writer, as much of it is in the line of confirming his views. Mr. Carter does not yet accept the theory of increased pressure toward the top, but if he will examine or experiment with heavy bracing in deep trenches in clear sand, or material with well-defined angles of repose, he will probably find much to help him toward the acceptance of this view.

The writer regrets that he has not now the means or appliances for further experiments with the piston chamber, but he does not believe that reliable results could be obtained in broken stone with so small a piston, as it is possible that the point of one stone only might be in contact with the piston. This would naturally leave the base exposed almost wholly to a clear water area. He does not believe, however, that in practice the laying of broken stone under inverts will materially change the ultimate pressure unless its cross-section represents a large area.

Mr. Perry will find the following on page 369:

"It should be noted also that although the area subject to pressure is diminished, the pressure on the area remaining corresponds to the full hydrostatic head, as would be shown by the pressure on an air gauge."

This, of course, depends on the porosity of the material and the friction the water meets in passing through it.

As to Mr. Thomson's discussion, the writer notes with regret two points: (*a*) that specific data are not given in many of the interesting cases of failures of certain structures or bracing; and (*b*), that he

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has not in all cases a clear understanding of the paper. For instance, the writer has not advocated the omission of bottom bracing or sheeting. He has seen many instances where it has been, or could have been, safely omitted, but he desires to make it clear that he does not under any circumstances advocate its omission in good work; but only that, in well-designed bracing, its strength may be decreased as it approaches the bottom. Mr. Meem.

Reference is again made to the diagram, Fig. 12, which shows that, in most cases of coffer-dams in combined aqueous and earth pressure, there may be nearly equal, and in some cases even greater, loading toward the bottom.

The writer also specifically states that in air the difference between aqueous and earth pressure is plainly noted by the fact that bracing is needed so frequently to hold back the earth while the air is keeping out the water.

The lack of specific data is especially noticeable in the account of the rise of the 6-ft. conduit at Toronto. It would be of great interest to know with certainly the weight of the pipe per foot, and whether it was properly bedded and properly back-filled. In all probability the back-filling over certain areas was not properly done, and as the pipe was exposed to an upward pressure of nearly 1 600 lb. per ft., with probably only 500 or 600 lb. of weight to counterbalance it, it can readily be seen that it did not conform with the writer's general suggestion, that structures not compactly, or only partially, buried, should have a large factor of safety against the upward pressure. Opposed to Mr. Thomson's experience in this instance is the fact that oftentimes the tunnels under the East River approached very close to the surface, with the material above them so soupy (owing to the escape of compressed air) that their upper surfaces were temporarily in water, yet there was no instance in which they rose, although some of them were under excessive buoyant pressure.

It is also of interest to note, from the papers descriptive of the North River Tunnel, that, with shield doors closed, the shield tended to rise, while by opening the doors to take in muck the shield could be brought down or kept down. The writer concurs with those who believe that the rising of the shield with closed doors was due to the slightly greater density of the material below, and was not in any way due to buoyancy.

Concerning the collapse of the bracing in the tunnel built under a side-hill, the writer believes it was due to the fact that it was under a sliding side-hill, and that, if it had been possible to have back-filled over and above this tunnel to a very large extent, this back-fill would have resulted in checking the sliding of material against the tunnel, and the work would thereafter have been done with safety. This is

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Mr. Meem. corroborated by Mr. Thomson's statement that the tunnel was subsequently carried through safely by going farther into the hill.

As to the angle of repose, Mr. Thomson seems to feel that its determination is so often impracticable that it is not to be relied on; and yet all calculations pertaining to earth pressure must be based on this factor. The writer believes that the angle of repose is not difficult to determine, and that observations of, and experiments on, exposed banks in similar material, and general experience in relation thereto, will enable one to determine it in nearly all cases within such reasonably accurate limits that only a small margin of safety need be added.

Engineers are sent to Europe to study sewage disposal, water purification, transit problems, etc., but are rarely sent to an adjoining county or State to look at an exposed bank, which would perhaps solve a vexed problem in bracing and result in great economy in the design of permanent structures.

Mr. Thomson's general views seem to indicate that much of the subject matter noted in the paper relates to unsolvable problems, for it appears that in many cases he believes the Engineer to be dependent on his educated guess, backed perhaps by the experienced guess of the foreman or practical man. The writer, on the contrary, believes that every problem relating to work of this class is capable of being solved, within reasonably accurate limits, and that the time is not far distant when the engineer, with his study of conditions, and samples of material before him, will be able to solve his earth pressure and earth resistance problems as accurately as the bridge engineer, with his knowledge of structural materials, solves bridge problems.

The writer, in the course of his experience, has met with or been interested in the solution of many problems similar to the following:

What difference in timbering should be made for a tunnel in ordinary, normally dry ground at a depth of 20 ft. to the roof, as compared with one at a depth of 90 ft.?

What difference in timbering or in permanent design should be made for a horizontally-sheeted shaft, 5 ft. square, going to a depth of 45 ft. and one 25 by 70 ft., for instance, going to the same depth, assuming each to be braced and sheeted horizontally with independent bracing?

What allowance should be made for the strength of interlock, assuming that a circular bulkhead of sand, 30 ft. in diameter, is to be carried by steel sheet-piling exposed around the outside for a depth of 40 ft.?

What average pressure per square foot of area should be required to drive a section of a 3 by 15-ft. roof shield, as compared with the pressure needed to drive the whole roof shield with an area four times as great?

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To what depth could a 12 by 12-in. timber be driven, under gradually added pressure, up to 60 tons, for instance, in normal sand? Mr. Meem.

What frictional resistance should be assumed on a hollow, steel, smooth-bore pile which had been driven through sharp sand and had penetrated soft, marshy material the bearing resistance of which was practically valueless?

What allowance should be made for the buoyancy of a tunnel 20 ft. in diameter, the top of which was buried to a depth of 20 ft. in sand above which there was 40 ft. of water?

It is believed by the writer that most of the authorities are silent as to the solution of problems similar to the above, and it is because of this lack of available data that he has directed his studies to them. The belief that the results of these studies, together with such observations and experiments as relate thereto, may be of interest, has caused him to set them forth in this paper.

He desires to state his belief that if problems similar to the above were given for definite solution, not based on ordinary safe practice, and without conference, to a number of engineers prominently interested in such matters, the results would vary so widely as to convince some of the critics of this paper that the greater danger lies rather in the non-exploration of such fields than in the setting forth of results of exploration which may appear to be somewhat radical.

Further, if these views result in stimulating enough interest to lead to the hope that eventually the "Pressure, Resistance, and Stability" of ground under varying conditions will be known within reasonably accurate limits and tabulated, the writer will feel that his efforts have not been in vain.

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AMERICAN SOCIETY OF CIVIL ENGINEERS

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TRANSACTIONS

Paper No. 1175

THE ULTIMATE LOAD ON PILE FOUNDATIONS: A STATIC THEORY.

BY JOHN H. GRIFFITH, ASSOC. M. AM. SOC. C. E.

WITH DISCUSSION BY MESSRS. LUTHER WAGONER, AND JOHN H. GRIFFITH.

Introduction.—In one of his discussions as to the ultimate bearing power of pile foundations, the late E. Sherman Gould, M. Am. Soc. C. E., stated that the theories of Goodrich had mathematically exhausted the subject, referring, of course, to a dynamic analysis. It is interesting, therefore, to note an entire departure from the usual procedure in a treatment proposed by Desmond* in which he studies a concrete pile purely by static methods.

A perfected static analysis would appear to have certain advantages over the older methods in that it will either eliminate altogether, or relegate to a sphere of minor importance, a number of elements the real significance of which, even in a most precise dynamic theory, is destined to be rather vague and indeterminate. One might cite, for example, the case where the pile bounds back or slowly rises after driving, owing possibly to a resiliency or sponginess of the soil, or perhaps to a buoyant effect of the latter on the pile. Such a phenomenon as brooming of the head might likewise be cited. When the engineer analyzes such perplexing problems as compression of the hammer or the pile, questions of impact, friction of the guides, measurements of velocity, and the like, the real import of any one of which will

* *Transactions*, Am. Soc. C. E., Vol. LXV, 1909. Discussion on paper, "Concrete Piles," p. 498, by Mr. Thomas C. Desmond.

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require involved analyses by the accomplished physicist, he may often be constrained to take the viewpoint of such eminently practical engineers as Haswell and Gould as to some of these matters. In fact, with any final working formula, to measure such an uncertain element as the penetration and neglect altogether the earth factors (as is tacitly done in any of the representative Sanders' expressions) would seem to seek a sort of negative magnification of the effect, reading, as it were, through the wrong end of the telescope, or taking observations at the short arm of the lever. Goodrich remarks* that:

"The liability to error is so enormous with small penetrations that no penetration should be trusted much less than 1 in., and no formula can be guaranteed within a reasonable percentage of error for less penetrations."

He shows that: "With a total penetration as large as 4 ins. (which is seldom observed), a variation of $\frac{1}{8}$ in. would make this penetration liable to 3% error."

Such a static theory will further endeavor to eliminate what Maxwell has called the historical element. The analysis of Desmond, for example, is not concerned with the load status a minute after driving, nor a year after, but rather in that indefinite period of time when the condition of the earth may be said to correspond with that minimum of stored energy which exists or tends to exist in Nature for stable equilibrium; or, if this element is to enter the analysis explicitly, it can only serve to render the problem more determinate. The dynamic analysis at best can only cover the situation in the period immediately after driving.

Then there are such formidable questions as the number of blows to refusal, the effect of the earth clinging to the pile, and many items of like moment.

In a larger sense, however, the static treatment should be viewed as complementary to the older method. A perfected theory of the pile will neither be confined exclusively to a study of the left-hand member of the equation of work, nor, in the other case, to the $\int Pds$ of the right-hand member, but, taking a unitary conception of the problem, will seek to include all variables and a determination of their effect on the status of ultimate load.

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* *Transactions*, Am. Soc. C. E., Vol. XLVIII, p. 205.

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It is to be hoped that Desmond's discussion may be the nucleus for a literature considering this larger view; further, that it may stimulate engineers to extend their experiments on earth pressures, hitherto confined to retaining walls, to include examinations of pile phenomena as well, the pile being in many respects a sort of inverted retaining wall in its analytical features.

The able engineers who have followed exclusively in the paths pioneered by Rankine and Moseley seem finally to have reached the proverbial blind alley in their attempts to solve the pile problem purely as a dynamic proposition; but Rankine* himself, it should be considered, at least implicitly suggested the static method in his attempt to figure the drawing power of screw-piles and the pressure on foundations. Any advance, however, in this field, seems to have been restricted, at least in America, by a too close adherence to his ellipse of stress principle, a rather subsidiary relation in the paper mentioned, which, while it may serve its purpose in elementary problems of the retaining wall, is not an efficient tool for a general investigation in the theory of earth pressure.

The writer will offer herein a few criticisms on the static method as it has been presented to date, and will outline some views as to its development along rational and empirical lines. In doing this, the paper will necessarily be confined to little more than an examination of the premises of the older authorities and an attempted statement of the problem. Owing to the scarcity of experimental data directly bearing on this subject, and an inadequate literature, such an investigation must be largely *a priori* in its nature, paving the way for a more rigorous analysis and suitable experimentation by others.

Existing Methods.—In the first and later editions of his "Civil Engineering" (1895), Patton gives the following equations for the "total bearing power of the pile":

$$P = A\, w\, x \left(\frac{1 + \sin.\, \phi}{1 - \sin.\, \phi}\right)^2 + \frac{S f\, w\, x}{2} \left(\frac{1 - \sin.\, \phi}{1 + \sin.\, \phi}\right) \text{ minimum,}$$

$$P' = A\, w\, x \left(\frac{1 + \sin.\, \phi}{1 - \sin.\, \phi}\right)^2 + \frac{S f\, w\, x}{2} \left(\frac{1 + \sin.\, \phi}{1 - \sin.\, \phi}\right) \text{ maximum,}$$

where w = the weight of a cubic foot of the material,
A = cross-section of the pile at the bottom,

* *Philosophical Transactions*, Royal Society, 1857.

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x = depth of the pile in the soil,
S = area of exterior surface of the pile,
f = coefficient of friction of earth on the pile surface.

The expression, $w\,x\left(\frac{1 \mp \sin.\,\phi}{1 \pm \sin.\,\phi}\right)$, is the intensity of lateral normal pressure, minimum and maximum, on the surface of the pile. When multiplied by the proper coefficient of friction of wood on earth, this resulting tangential stress, when summed over the whole peripheral surface of the pile, gives, according to the Patton theory, the frictional resistance of the soil. The first terms in the right-hand members of each equation give the pressure on the base. Patton remarks:

"If proper values of ϕ, S, and f in equations above are determined by experiment, it would seem that these formulæ would produce better and more reliable results than the more common formulæ would."

The solution given is the earliest direct attempt to solve the problem (other than that given by Rankine, before mentioned) that has come to the writer's attention.

Very recently, Professor Vierendeel (University of Louvain, Flanders) has treated the subject in more detail, together with the dynamic method, in a comprehensive work* in which he gives the formula:

$$R = \pi\, D\, f\, w \frac{1 + \sin.\,\alpha}{1 - \sin.\,\alpha} \frac{L^2}{2} = 1.5\, D\, f\, w\, L^2 \frac{1 + \sin.\,\alpha}{1 - \sin.\,\alpha},$$

which he deduces by the principle of work, where R = the ultimate load, D = the mean diameter of the pile, L = the depth of penetration, w is the unit weight, and α is the natural talus.

It will be seen by a little study that the foregoing methods are practically in agreement with the aforementioned treatment by Desmond, in that each makes use of the ordinary Rankine relation, multiplies by a friction factor, and integrates the stress in one form or another over the entire surface in contact with the soil.

Viewed as an empirical expedient, such equations should commend themselves to engineers for practical use in fixing load limits. In this capacity, they will doubtless excel the ordinary Sanders' energy formulas, if constants are properly evaluated from test loadings, as suggested by Patton.

* "Cours de Stabilité des Constructions" (Tome VI, 1907).

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A true empirical basis for the study of the pile problem may be established by actual laboratory tests more easily than in the case of the retaining wall; for if loads at incipient motion are measured on a model pile which passes entirely through a reservoir of sand, having a hole in the base for egress of the pile, actual values of the total peripheral friction may be obtained and studied with respect to its variation for a variety of perimeters. Combined effects of basal and lateral stress could be obtained, of course, by independent experiments. It is important, however, that the base and lateral effects should be differentiated if they are to be studied and analyzed.

If, however, the methods given by these authors are to be construed as rational propositions, then, in their present form, they appear to be open to serious criticism, because, in making use of Rankine's expression for the intensity of stress, they violate his principle of conjugate stresses, which in this particular case makes the expression of the form, $w\,x\left(\frac{1 \mp \sin.\,\phi}{1 \pm \sin.\,\phi}\right)$, a principal stress, that is, one purely normal to the surface of the pile and having its maximum value. Consequently, the notion advanced by these writers of multiplying this principal stress by a friction factor is incompatible with the well-known principles of mechanics of stress.

Empirically, however, there is as much justification for the use of such types of formulas as there is for any of the present-day column formulas or some of the beam applications. The forms of the expressions are correct enough, as far as Rankine's intensity of lateral pressure is concerned, but, of course, the angle, ϕ, must be considered as an arbitrary parameter to be determined for certain soils, and not as the angle of repose or internal friction. Just what the deviation of this parameter from the angle of internal friction will be must be determined by such experiments as have been suggested or by actual tests in the field for ultimate loading.

A general criticism, of course, is that the problem in its final analysis will not lend itself to any such elementary forms as a Rankine solution may be expected to give. Any theory must experience that evolution characteristic not alone of the dynamic analysis of the pile and the retaining wall, but of all the classical problems in engineering. In such an evolution the Rankine theory rightfully assumes its place as a primitive, true enough under its own premises, but of

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which the premises are not general enough to include the whole range of phenomena either of the pile or of the retaining wall.

The Rankine Theory.—In view of the fact that the Rankine theory has already taken its place as the basis for a static analysis of the pile, it is important that it be rigorously stated. The following is conceived to be an exact solution, with no assumptions except those contained in Rankine's premises.

Consider a pulley-shaped foundation, with data as indicated in Fig. 1, which, as in the case treated by Desmond, may be a concrete or timber pile jetted or driven to place. Any form of cross-section might be taken, but, for simplicity, it is assumed as circular.

The dotted lines may be considered to represent displacement filaments passing out from the horizontal rims to the free surface around the head of the pile. The position of these lines can only be inferred from the treatises, say Ketchum's or Vierendeel's, as few if any precise investigations have been made along this line.

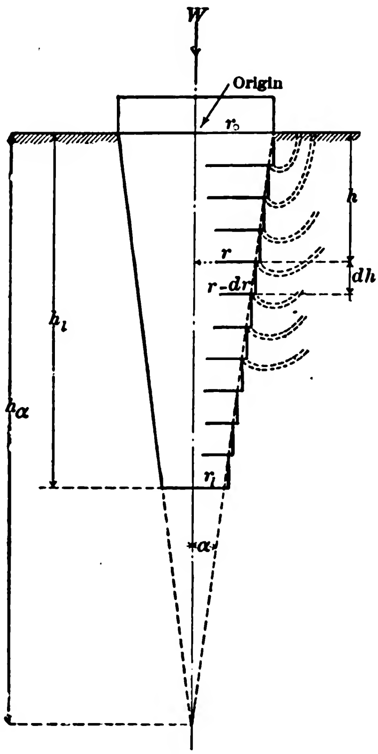

FIG. 1.

At incipient motion of the pile, it being assumed that it is at its final depth, any increment of the load will cause an actual displacement of the particles, and this will manifest itself as an increment or surface displacement to the upheaval surface which has formed around the head of the pile in driving. This assumption is necessary under the Rankine hypothesis of incompressible particles, although it has been the writer's experience that the phenomenon is often difficult to observe at such a stage. The load at this time is considered to be the ultimate carrying capacity, by the Rankine law.

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The area of a small rim of variable radius, r, and width, $dr = 2 \pi r\, dr$.

Let p = the intensity of pressure on this rim element.

Then $p = wh \left(\frac{1 + \sin. \phi}{1 - \sin. \phi}\right)^2$ for a maximum,

where w = the weight of a cubic unit of earth,

and ϕ = the angle of internal friction, assumed as constant.

The total pressure on the element $= 2 \pi r\, dr \left(\frac{1 + \sin. \phi}{1 - \sin. \phi}\right)^2 wh.$

Now substitute $r = (h_a - h) \tan. \alpha$,

and $dr = - \tan. \alpha\, dh$,

where h_a represents the distance from the surface to the vertex of the cone formed by the surface of the pile, h_l = the actual length in the earth, and α = the angle of slope of the conical surface. The total pressure on the rim element becomes

$$2 \pi w \left(\frac{1 + \sin. \phi}{1 - \sin. \phi}\right)^2 \tan.^2 \alpha \int_{h_l}^{o} - (h_a - h)\, h\, dh.$$

In order to take account of a principle of continuity, which in this case will manifest itself in the law of pressure varying as a function of the depth, one may conceive that, as the elementary rim pressure exceeds the amount above given, the pile will tend to subside under this, so that each rim will take its proportionate quota of stress in turn. The total buoyant effect is at the limit when the pulley-shaped foundation becomes a conical-shaped pile. The value of the integral becomes:

$$\int_{h_l}^{o} - (h_a - h)\, h\, dh = \left[- \left(h_a \frac{h^2}{2} - \frac{h^3}{3}\right)\right]_{h_l}^{o} = h_a \frac{h_l^2}{2} - \frac{h_l^3}{3},$$

and, substituting this in the previous expression,

$$P_{(lat.)} = 2 \pi w \left(\frac{1 + \sin. \phi}{1 - \sin. \phi}\right)^2 \tan.^2 \alpha \left[h_a \frac{h_l^2}{2} - \frac{h_l^3}{3}\right],$$

where the expression, $P_{(lat.)}$, represents the entire upward pressure on the lateral surface of the pile. To this must be added the basal pressure, giving, for the total load, P, which the pile can sustain according to Rankine's theory:

$$P = 2 \pi w \left(\frac{1 + \sin. \phi}{1 - \sin. \phi}\right)^2 \tan.^2 \alpha \left[h_a \frac{h_l^2}{2} - \frac{h_l^3}{3}\right] + \pi r_l^2 w h_l \left(\frac{1 + \sin. \phi}{1 - \sin. \phi}\right)^2.$$

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In the case of the "butt end down," the weight of the variable column of earth may be similarly summed and added to the load on the pile, and this equated to the bearing power of the base.

Such an analysis assumes, of course, that the earth conditions, absence of cohesion, etc., will warrant a treatment by the Rankine method. It is believed to give all that can consistently be demanded of the hypothesis.

Limitations of the Theory.—It will be seen that the above application is quite limited in its efficiency as a working method. Specifically, it neglects the friction on the vertical projections of the face. Indeed, the Rankine premises do not take cognizance of any foreign body, such as the pile, but confine the problem to an indefinite extent of the material.

While it assumes the existence of displacement tubes, it makes no analytical provision as to their zone of action, unless one may take any series of vertical and horizontal lines as defining the field.

The usual applications of this theory assume a constant coefficient of friction, which, in the light of experiment, is only approximately tenable; but, confining the problem to its own more particular domain, the chief limitation is the necessity of the assumption of Moseley's law of least resistance as Rankine referred to it, at once either the element of weakness or of strength in his method, as one may prefer to call it.

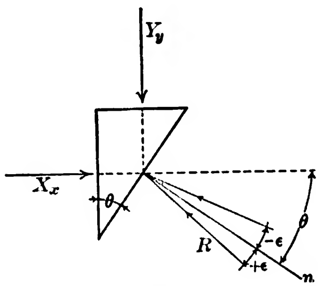

FIG. 2.

Consider an ordinary wedge element of the material, Fig. 2, with vertical and horizontal faces and an inclined face the normal of which, n, is inclined at an angle, θ, with the horizontal. The area of this θ-face may be conveniently taken as unity.

Let the intensity of the vertical stress be considered in this particular case as due to a column of earth of length y feet below the surface of the ground, the value of which is Y_y. The corresponding intensities upon the x- and θ-planes, respectively, are X_x and R. The stress, R, has an obliquity of $\pm\ \epsilon$ from the normal. By compounding stresses, by any of the elementary methods, there results the general expression:

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$$X_x = \frac{\tan.\ \theta}{\tan.\ (\theta \pm \varepsilon)} Y_y.$$

To evaluate X_x, another condition is required. Rankine sought to supply this condition through the Moseley assumption, taking the obliquity, $\pm\ \varepsilon$, as having its maximum value, ϕ, at impending motion of the particles. By seeking the maximum and minimum values of $\frac{\tan.\ \theta}{\tan.\ (\theta \pm \varepsilon)}$ on this basis, there results then, for the particular values of θ where Rankine's value of X_x may be assumed to hold:

$$\theta = \text{multiples of } \frac{\pi}{4} - \frac{\phi}{2}, \text{ for } X_x \text{ a maximum,}$$

$$\theta = \text{multiples of } \frac{3\ \pi}{4} - \frac{\phi}{2}, \text{ for } X_x \text{ a minimum,}$$

for positive values of ϕ, and in a similar manner when ϕ is negative. For example, taking a common value of $\phi = 30°$, one receives $X_x = \frac{1}{3}\ Y_y$ and $3\ Y_y$, as in the ordinary case. For the above given values of θ, Rankine's solution may be considered to hold, but for all other values the problem is absolutely indeterminate. The common practice of engineers, in applying this method as a general solution to problems of earthwork, is quite in keeping with that practice which seeks the deportment of a column within the elastic limit from tests to destruction.

Neither will the common defense, of the law being on the safe side, hold in all cases. For instance, it has already been pointed out by Boussinesq* that, in the case of a retaining wall when it is in its ordinary position of equilibrium, otherwise than at the time of incipient motion, as predicated by Rankine, although the particles are less forcibly retained, they nevertheless exert upon the structure a greater thrust than that given by Rankine.

A number of practical phases of this indeterminateness might be cited, showing the shortcomings of the method as a theoretical device. This is made apparent in the packing of balls. For example, a rather low angle of repose may be expected for fine shot if it is dropped from a short height, but had one the patience to arrange the shot particle by particle in a pyramidal array, according to the geometry of packing spheres, a much higher angle might be obtained

* "Essai théorique sur l'équilibre des massifs pulvérulents, comparé à celui de massifs solides, et sur la poussée des terres sans cohésion," (1876), p. 5.

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for the slope of the pyramid, and this would be entirely independent of the condition of the balls, that is, whether rough or frictionless. Further, taking the old problem of the thousand 1-in. balls* packed in cubical array in a 10-in. cubical box, it is quite possible to conceive of an angle of repose of 90° if the sides of the box could be gently removed, although, of course, in such a case, the equilibrium would be very unstable. In the latter cases, the Moseley assumption would be quite justifiable. However, taking another extreme, say, the thrust of barrels on the walls of a warehouse, only the exigency of an occasional earthquake could render the application of the method theoretically permissible. The law is inoperative.

It is such limitations as have been cited that render the Rankine method of rather doubtful utility for any general rational treatment, either of the pile or the retaining wall. European and other than American authorities have ceased treating the Rankine formula as a general solution for all problems involving the lateral pressure of earth, and prefer to give it its more proper position as defining one particular kind of equilibrium. Even in its own special field, a solution approaching nearer the facts may doubtless be secured in many cases by the more determinate method of Greenhill,† as in the instance of barrel thrust.

Theoretical Position of the Method.—In order, then, to give to the Rankine theory applied to the pile that definiteness of position which attaches, say, to that of Euler's formula in the column theory, it may be defined as the theory of an infinitely smooth shaft afloat on a medium deporting in several respects as a sort of generalized fluid, where the particles are subject to negative normal stresses or pressures and to tangential or friction stresses, but where no permanent shearing resistance exists. In such a theory the vertical pressures may be assumed to follow the hydrostatic law. The horizontal pressures will also follow this law, but, owing to friction, the effect is such as would occur with a reduced specific weight, $w\left(\frac{1 \pm \sin.\ \phi}{1 \mp \sin.\ \phi}\right)$, where ϕ is the angle of internal friction, or, as Rankine referred to it usually, the angle of repose. The ($\pm$) signs are to be used in the above for the case of maximum loadings, in

* Quoted by Greenhill from "Cosmos," September, 1887, "Hydrostatics," p. 52.

† Greenhill's "Hydrostatics," pp. 45 *et seq.*

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which case the pressure exerted by the pile is a so-called "active force," as the term is used by Rankine. The ($\mp$) signs are for minimum loads on the pile, namely, if the "buoyancy" of the surrounding earth (viewing this now as an active force) is greater than the load on the pile, as prescribed by this theory, the pile will tend to rise, and may actually do so, especially if the medium contains more or less water.

Accordingly, it will be seen that the laws of pulverulent masses will agree well with the theory originally advanced by Boussinesq,* and given later by Flamant,† Greenhill,‡ and others, in that they are intermediate in their properties between fluids and solids. Fresh cement, in its ordinary condition, will follow closely the hydrostatic law, but, under pressure, will take on the properties of elastic bodies. Even the Rankine equations, if consistently interpreted, find analogies in the theory of stress and strain in solids on the one hand (Tresca), and agree with the hydrostatic law for $\phi = 0$, on the other.

A dynamics of pulverulence is quite possible to formulate under such a notion, and would probably find practical applications in designing orifices for the discharge of grain, etc. Under this caption such phenomena as have been described by Vierendeel§ as occurring at the circumference of disk piles, and by Le Conte‖ and Goodrich,¶ as "cones" and the like, forming under the bases of models, would probably find interpretation as suppressed vortex or eddy effects.

Practical Utility of the Rankine Formula.—While the Rankine theory is little more than an abstraction, and if consistently and rationally applied to a single pile can only be expected to give a fraction of the real carrying power, its utility to the practicing engineer may still exist in the fact that a multiple-pile system may be tested by Rankine's equations about as logically as they may be applied to any ordinary foundation. In such a multiple pile the integrity of the structure is usually preserved by suitable framing; but, if this were not so, the material in the cusp-like interstices between the piles can be expected to be much more compressed, and consequently to have a considerably

* "History of the Elasticity and Strength of Materials," Vol. II, Pt. II, Article on Boussinesq, by Karl Pearson.

† "Stabilité des Constructions," p. 111.

‡ "Hydrostatics," pp. 45 *et seq.*

§ "Cours de Stabilité des Constructions," Vol. VI, p. 246.

‖ *Transactions*, Am. Soc. C. E., Vol. XLII, p. 284.

¶ *Transactions*, Am. Soc. C. E., Vol. XLVIII, p. 181.

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higher friction factor, than the less restrained material at the periphery of the composite structure, thus tending to maintain this unity of action.

In a multiple pile great reliance is placed on the increased density of the soil, due to the driving, with the corresponding increase in the friction coefficient. As the condensation under the Rankine premises is purely inelastic, an approximate idea of the increase in density may be found by an equation between the displacement of the pile and the upheaval mass around the head.

Nearly all writers, with the exception of Vierendeel, in discussing the bearing power of foundations, follow Rankine in ignoring the stresses on the side walls, and confine their analysis solely to the base. Accordingly, on the common theory, a designer of a multiple pile would neglect the peripheral friction on the composite structure in comparison with the presumably larger pressure on the base. In this case such a procedure can be viewed as giving only crudely approximate results. It is believed that the phenomenon of dilatancy of media composed of rigid particles, as studied by Professor Osborne Reynolds,* may even warrant the belief that this lateral friction is larger than supposed, especially in water-bearing strata. The writer will revert to this point later.

The Elastic Theory.—Nearly all the structural problems of engineering find their ultimate analysis in the elastic hypothesis. This is true of the arch, and in a large measure of the retaining wall. Just as the beam, on account of the labors of de St. Venant and his contemporaries, owes its truly rational position to such elastic studies, quite independent of the empiricists, the column theory, with of course a few possible exceptions, may be said to have made no consistent advances since the days of Euler by departing therefrom.

While to place such an apparently crude and sordid problem as the pile in this field will undoubtedly seem inopportune, it is believed that, in the end, such a step will avoid a great deal of useless effort and incorrect thinking. It is thought important to bring out a few arguments *pro* and *con* as to the advisability of such procedure.

In the first place, to make the problem of the lateral pressure of earth truly determinate, the idea of strain is involved. Its introduc-

* *Philosophical Magazine* (London, E., and D.), Vol. XX, 1885, p. 469. "On the Dilatancy of Media Composed of Rigid Particles in Contact;" also Reynolds' Works, Vol. III.

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tion into the analysis is due to Boussinesq. Take the case previously cited, of the problem of the balls packed in the 10-in. box in cubical array. The problem of their lateral pressure against the walls owing to their own weight becomes at least theoretically determinate, provided all the stress and strain constituents of the material are known, and the elastic deportment of the walls is understood. Although such elastic solutions are in many cases extremely difficult to obtain, on the other hand, they have the advantage of a high degree of certainty of result, and will tend to obviate that endless modification so common, say, in column and pile formulas.

As contributing data toward such a final and correct analysis, ideal problems, approximating in part toward the actual conditions, may be solved. For example, it may be shown that:

"If a vertical cylindrical hole of circular section is cut in a rigid body, and an elastic cylinder of density ρ, which, if freed from the action of gravity, would exactly fit the hole, is placed in it and stands upon the bottom, * * * the sides of the hole suffer the same hydrostatic pressure as if it were filled with a liquid of density $\rho\ (m - n)\ (m + n)$." (Ibbetson.)

Slichter,* in commenting along this line, remarks:

"It is important * * * that we should have before us the solution of as many problems as possible, since the most likely method by which we shall be able to solve a new problem is by reducing it to one of the cases in which a similar problem has been constructed by the inverse process. Indeed, one must often be content to secure an approximate solution in a given case by searching among problems already solved for one whose equipotential lines or surfaces have a form somewhat resembling the given boundary, and then so to modify the problem by tentative methods as to produce conditions more nearly corresponding to those of the given problem. For this reason it is desirable to solve all possible kinds of problems * * * whether they seem to be 'practical' or not."

Accordingly, Coulomb, Rankine, Weyrauch, Levy, Boussinesq, Kötter, and others, have contributed much in their study of various kinds of equilibrium. The work of Boussinesq, while furnishing valuable researches in the whole field, seems to be carefully ignored by the practicing profession.

Such an elastic hypothesis, it has been urged, is less applicable to

* "Theoretical Investigation of the Motion of Ground-Waters," by C. S. Slichter (Government Printing Office, Washington, D. C., 1899), p. 333.

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the case of earth pressure than in the case of any other medium, it being difficult to predicate continuity laws of the medium and the existence of derivatives, as is done in hydrodynamic and elastic theories. As sufficiently typical of such criticism, the remarks of Darwin* are closely to the point.

"It has always been assumed by previous writers that the tangential action across an ideal interface in a mass of loose earth is of the same nature as the statical friction between solids, and that when the tangential stress has attained in magnitude a certain fraction of the normal stress, the equilibrium is on the point of breaking down. * * * A little consideration will show that the hypothesis cannot be exact, even with an ideal sand with incompressible grains, and absolutely devoid of coherence. For imagine a mass of sand thrown loosely together; then if the grains are of irregular shape a certain portion of them will be resting on points and angles, thus occupying more space than they might do.

"If the sand be now compressed, many of the grains will slip and rotate, and fall into interstices; in fact a considerable amount of re-arrangement will take place, and the density of the mass will rise considerably—by quite 10 per cent. if the re-arrangement be thorough, as found experimentally.

"Even if all the grains were spherical a considerable amount of change would take place, and when they are angular of course much more. * * *

"Hence it is clear that the coefficient of internal friction of sand is a function of the pressure, and not merely of the pressure then existing, but also of the pressure and shaking to which at some previous period that portion of the mass of sand has been subjected. * * *

"It is quite impossible to say how much these causes will vitiate any mathematical theory of the equilibrium of sand, but experience seems to show that the vitiation is extensive."

On the other hand, in the elastic theory, the researches of Boussinesq† show that pulverulent material when under presure—such as may occur in this particular case of the pile owing to impacted soil through driving, even more than in the retaining wall—resists a change of form with a force which is proportional to the mean of the three principal stresses acting on the particle. He takes the coefficient of rigidity, μ, as varying with this mean pressure. As the weight on the particle increases, either owing to its own "head," or, in this case, to

* *Minutes of Proceedings*, Inst. C. E., Vol. LXXI, 1883, "On the Horizontal Thrust of a Mass of Sand," pp. 374 *et seq.*

† "Essai théorique * * *," p. 6.

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the compressed soil in driving, the surrounding medium approaches an elastic body in its properties. Under great pressure, of course, it becomes perfectly so, thus justifying geologists or physicists in calculating earth stresses, delta pressures, faults, etc., by known elastic methods.

Now, the writer believes that there is a tacit notion, prevalent among representative engineers, which is quite conformable to such an hypothesis, and in support of this belief would quote the remarks of Goodrich:*

"When a pile is supported entirely by the frictional resistance, the actual region supporting the load is some deep ground level at which the frictional resistance holding the pile has been transferred through the earth in the shape of a conoid of pressure, the base of which gives a total bearing value equal to the load and a unit bearing value which the earth at that lower level will support. Each kind and degree of compactness of earth will give a different angle for the slope of the conoidal surface."

Again, he says:

"When supported by frictional resistance, they [the piles] must be driven so far apart, or to such a depth, that the increased area of bearing developed by the conoid of pressure having the required altitude of frictional resistance meets a level which will afford the required support before intersecting the conoid of a neighboring pile."

Such a description would seem to show analogies with the "fan" distribution of Stokes and Carus Wilson,† with the local perturbations of Boussinesq,† or some of the equipollent effects of de St. Venant.

It is natural to ask, however, how the inelastic distortions of Darwin can be made to harmonize with the other views. The answer would be by postulating or defining the medium. Slichter,‡ in a somewhat related problem involving a study of the flow of groundwaters through a soil, has attacked his problem very successfully by the assumption of a mean soil. The size of the grains in a soil having the same transmission power as the more complex soil he calls the "effective" size. He says:

"There probably exists a tendency in every such soil toward a certain average size and mean arrangement of grains which the theory

* *Transactions*, Am. Soc. C. E., Vol. XLVIII, "Supporting Power of Piles," pp. 182 *et seq.*

† *Proceedings*, Physical Society of London, Vol. XI, 1891, p. 194, "The Influence of Surface-Loading on the Flexure of Beams."

‡ "Theoretical Investigation of the Motion of Ground-Waters," p. 305.

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of probabilities would justify us in setting up as an ideal soil to replace a given soil in the investigation."

The same remarks may be applied to the analysis of the pile and related phenomena. It is this idealization of the problem which is tacitly done in all the problems of engineering, perhaps, however, with less justification at this time in the theory of earth pressure, on account of the lack of physical investigation.

On the whole, the opinion of elasticians, Darwin and de St. Venant included, would seem to be favorable to an elastic analysis of the problem of the lateral pressure of earth and pulverulent material. Pearson,* in his critique of the elastic analysis of Boussinesq, has said:

"They appear to contain the most complete scientific theory yet given of the stability of such a mass * * * indeed, they are perhaps the limit to what elastic theory can provide in these directions."

In view of the dearth of knowledge of strain and friction factors, little progress can be made. It is believed, however, that as engineers direct their attention to the static outlook and conduct experiments along this line, a great many features now rather obscure will clear up. Such a study also affords another angle of vision upon the pile viewed dynamically and the retaining wall.

General Notions.—For purposes of discussion, consider a pile driven or jetted to place and carrying, say, its maximum load. It is desired to investigate the mechanical state of the soil as it reacts upon the pile and prevents its further subsidence under the load. The principles of mechanics† furnish the following well-known equations for the static equilibrium of a volume element of the material surrounding the pile, say, a small parallelopiped the co-ordinates of which, as shown in Fig. 3, are x, y, and z.

$$\frac{\delta X_x}{\delta x} + \frac{\delta X_y}{\delta y} + \frac{\delta X_z}{\delta z} + \rho X = 0$$

$$\frac{\delta Y_x}{\delta x} + \frac{\delta Y_y}{\delta y} + \frac{\delta Y_z}{\delta z} + \rho Y = 0$$

$$\frac{\delta Z_x}{\delta x} + \frac{\delta Z_y}{\delta y} + \frac{\delta Z_z}{\delta z} + \rho Z = 0$$

Using the Kirchoff notation, as preferable to that of Lamé, the expression, X_x, represents the intensity of normal stress on the elementary area, $dy\ dz$, of the parallelopiped, that is, the stress acting

* "History of Elasticity and Strength of Materials," Vol. II, Pt. II, pp. 818 and 857.

† "Theory of Elasticity," by Love, Chapter V, pp. 122 *et seq.*

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in the direction of the x-axis upon the plane element, $dy\ dz$, perpendicular to this plane. Briefly, X_x represents the x-stress upon the x-plane. In a similar manner, X_y is the x-stress on the y-plane, a shearing or tangential stress; Z_z is a normal stress upon the z-plane, and so on. Since the shears at right angles are always equal, then $Y_x = X_y$, $Z_x = X_z$, $Z_y = Y_z$; but, for convenience of mental retention, the symmetrical notation commends itself. The equivalence can be asserted as desired in calculations of any particular problem.

The expressions, ρX, ρY, and ρZ, where ρ is the density, represent "body forces," such as gravity or "centrifugal" force. In this case, these volumetric forces may be the components of gravity in the direction of the co-ordinate axes, or, as these are taken in Fig. 3, $\rho X = \rho Z = 0$, and $\rho Y =$ the weight of the earth per cubic foot in the engineer's notation. For sand charged with water, this would be, say, 110 lb., or as the case might be.

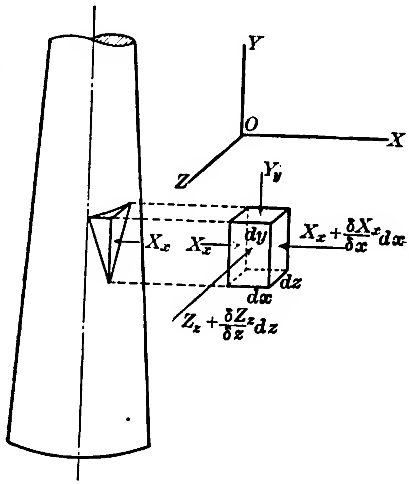

FIG. 3.

The foregoing equations, as used by Rankine* in his original paper, are rigid body equations. Boussinesq,† by introducing a comprehensive theory of strain, formulates an independent system for the theory of earth pressure. There are, of course, relations of compatibility in the general problem which will show analytically, as already shown for the Patton equations, that the engineer cannot choose his values at random.

Thus far, as the writer has discovered, few practical data in the matter of strain are accessible for different earths. Reference to this must be brief. The theory, then, will be founded on stress relations, as in the ordinary beam formula, for practical purposes.

In the case of conical or cylindrical piles, the equations for static equilibrium in a final analysis will be best expressed in the well-

* *Philosophical Transactions*, Royal Society, 1857.

† "Essai théorique sur l'équilibre d'élasticité des massifs pulvérulents * * *," p. 24, etc.

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known cylindrical co-ordinates, the notation being similar to that used before, namely:

$$\frac{\delta Y_y}{\delta y}+\frac{\delta Y_r}{\delta r}+\frac{\delta Y_\phi}{r\,d\,\phi}+\frac{Y_r}{r}+\rho\,Y=0$$

$$\frac{\delta R_y}{\delta y}+\frac{\delta R_r}{\delta r}+\frac{\delta R_\phi}{r\,d\,\phi}+\frac{R_r-\phi_\phi}{r}+\rho\,R=0$$

$$\frac{\delta \phi_y}{\delta y}+\frac{\delta \phi_r}{\delta r}+\frac{\delta \phi_\phi}{r\,d\,\phi}+\frac{\phi_r+R_\phi}{r}+\rho\,\phi=0$$

In these equations the capital letters give the direction of action of the stress and the subscripts refer to the planes on which they act. For example, ϕ_y represents the intensity in the direction of the normal to the plane, *dr dy*, on the *y*-plane, that is, the plane, *dr r dϕ*, etc. The shears in rectangular directions, as in the previous case, are equal.

In this more complicated case, however, owing to the symmetry about the *y*-axis, or axis of the pile, the "hoop compression" becomes constant around any particular ring element of the radius, *r*. The shears also vanish on the ϕ-planes, that is, any of the faces, *dy, dr.* This distribution of stresses, when a solution of the various particular intensities is obtained, may ultimately be used for obtaining the tubes of stress, their intensities and slopes at any point, in the conoid described by Goodrich.

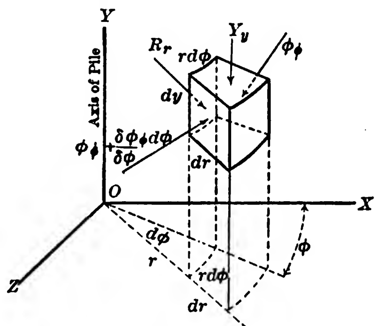

FIG. 4.

Now, to make any particular problem a determinate one, such types of equilibrium equations as have been given are to be satisfied for all values of the variables and for certain boundary conditions, namely, at the upheaval surface and at the entire periphery of the pile. In a continuous beam the analogy exists in the state at the supports. Similarly, a correct column formula must not only satisfy such equilibrium equations along the axis, but also hold for very long and very short columns. The problem under discussion is relatively more determinate than in the column problem, as the best that may ultimately be expected in the latter case is a least-square solution.

At the surface of the pile the following type of equations must be satisfied, as well as for the upper surface.*

* "Theory of Elasticity," by Love, Chapter V, pp. 122 *et seq.*

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$$X_n = X_x \cos. (x\,n) + X_y \cos. (y\,n) + X_z \cos. (z\,n)$$
$$Y_n = Y_x \cos. (x\,n) + Y_y \cos. (y\,n) + Y_z \cos. (z\,n)$$
$$Z_n = Z_x \cos. (x\,n) + Z_y \cos. (y\,n) + Z_z \cos. (z\,n)$$

To make these expressions clear, it may be remarked that the surface of the pile, being in the general case the surface of a cone, will transform the volume element of earth, $dx\ dy\ dz$ (Fig. 3), into a tetrahedral element. And these equations assert the equilibrium of all stresses on the tetrahedron in the directions, x, y, and z, respectively.

Call the surface element of the pile, that is, the inclined face of the tetrahedron element, the n-face, because its normal is, say, n. Let its area be unity, for convenience of discussion. Then the other faces, namely, the x, y, and z-faces, respectively, are cos. (xn), cos. (yn), and cos. (zn), where (xn), (yn), and (zn) are the angles between the x, y, and z-directions and the normal of the n-face or n. Accordingly, X_n is the resultant stress component in the x-direction on the surface element of the pile. A similar set holds for the ground surface, but becomes very much simpler owing to vanishing of terms when the upheaval surface is assumed as horizontal.

Now, in a precise and finished analysis involving the strain relations, both the boundary equations just given and the equilibrium equations are usually expressed in terms of these strains. Just as they are neglected in the derivation of the beam formula, they will be neglected here. The two sets of equations will be used solely as stress relations, as given, to keep the problem within working bounds.

A two-dimensional solution only can be attempted at this time, on account of the analytical difficulties involved in the more general treatment. It is believed, however, that a general solution exists in the case where the "immersed" length of pile is zero in the Boussinesq* problem of the distribution of stress and strain due to a rigid cylinder resting upon an infinite elastic solid, combined, of course, with suitable superpositions to provide for the weight of the soil. Moreover, since the strain in the earth at some distance from the body is quite independent of the manner of distribution of the peripheral stresses, but will depend rather on the resultant statically

* "Application des potentiels à l'étude de l'équilibre et du mouvement des solides élastiques," 1885.
"History of Theory of Elasticity," Todhunter-Pearson, Vol. II, Pt. II, p. 237.
Love's "Theory of Elasticity," Chapter VIII.

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equivalent to them, it is thought that this solution for immersion of length zero may actually be taken for finite lengths of the pile. It would seem to the writer that the existence of the "cone" under the base will approximately justify this.

All authors, from Barlow and Rankine to the present time, have pleaded a lack of experimental data with which to correlate their mathematical investigations. The writer has felt this constraint in his attempts to get any trustworthy results from the case given, after analyzing the problem from different points of view; but, while these efforts have been largely fruitless, they have afforded certain lines of approach in analyzing the "conoid."

One of these is that, in the case of experiment, instead of restricting the investigation solely to the special case of granular or pulverulent media, as all engineers have heretofore done, the problem should be generalized to include media which have elastic properties within limits, say clay, hardpan, spongy soils, and very probably sand in its most compact position, especially when it is charged with water. It is believed that, eventually, when more experiments have been made, these premises will be easier to work to than in the case of granular media. In some preliminary experiments along this line, made for the purpose of throwing light upon more precise efforts to be undertaken, C. J. Green, Jun. Am. Soc. C. E., and the writer used rather fine and compacted saw-dust, in a duplication of the Goodrich* experiment made with sand. Such a saw-dust medium will permit a considerable magnification of the strain that may be expected in an actual case, when a small vertical motion of the model pile is made in the medium, keeping the "pile" close to the glass wall of the box. Leygue,† in his experiments on retaining walls, used a series of strata of a different colored medium to bring out the faults in the sand and confirm his notion of a curved surface for the interior face of the Coulomb wedge. In like manner, this notion has been tried by "sprinkling" a series of co-ordinate lines of any convenient medium on the face of the glass wall when laid flat with the "pile" in place, and laying over this the saw-dust, with a view of showing the strained lines when a small vertical displacement of the "pile"

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* *Transactions*, Am. Soc. C. E., Vol. XLVIII, p. 181.

† *Annales des Ponts et Chaussées*, 1885.

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occurs. The original positions of the co-ordinates are marked on the glass with a wax pencil.

Two limiting aspects are to be studied: First, the strained condition for a very smooth or polished prism with a flat base, and then that for one with serrated or notched faces next to the saw-dust. The first case simulates that where the pressure on the sides is normal. The second case approximates the actual status of a pile in a cohesive soil where the full friction exists. While little of this has been carried out, it is believed that qualitative data of value will be obtained by using, not only straight prisms, but also wedges of rectangular cross-section with the faces next the material inclined to and from the vertical. It is hoped in the first case to obtain the deportment of the material under the pile. Preliminary experiments seem to confirm, partially at least, such a flow of stress as has been already derived both experimentally and analytically by Hertz* in the well-known problem of the pressure between two elastic bodies in contact. It is thought, by carrying out the Goodrich experiment as thus described, not only for sand, but also for other "more springy" media, that a great deal of light may be afforded, not only on the basal action of the pile, but also on the related problems of surface loading, as in beams, etc. Here, analysis is already far ahead of experiment, at least for elastic bodies.

In the second case, it is desired to discover the zone of action in regard to the lateral friction in a cohesive and elastic soil. In the subsequent analysis this can only be assumed for the case of pulverulent material.

Two-Dimensional Stress Relations.—With the Rankine premises the uniplanar or two-dimensional case is easily extended to three dimensions by the assumption of a vertical axis of symmetry, namely, his ellipse of stress relation becomes an ellipsoid of stress; but, when the influence of a body such as the pile is concerned, the problem becomes greatly complicated, involving a solution in the case of stress alone of the equations of equilibrium in cylindrical co-ordinates subject to proper boundaries, as has been shown. The writer has been unable to obtain general solutions for these, as has been already remarked.

*Hertz, "Miscellaneous Papers," Translation by Jones and Schott.
Hertz, J. F. Math. (Crelle), Bd. 92 (1881).
Love's "Theory of Elasticity," p. 195.

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It is proposed, in accordance with the suggestion of Slichter, to attempt an approximate solution as the best available at this time. Such a solution, accordingly, may be considered to be a second approximation to that already given by Patton and by Desmond, but it will avoid largely the Rankine inconsistencies. This may then be used in studying the experimental data at hand with a view to discovering the general law, if such law does not already exist, at least for short piles in the Boussinesq problem of the rigid cylinder.*

In a two-dimensional case, either of the sets of equilibrium equations may be applied as it were to a pile of very large radius, or, taking as equivalent, a stretch of sheet-piling. Accordingly, the piling partakes more or less of the nature of the retaining wall.

Two-dimensional treatments of the equilibrium equations have already been given, in the case of the retaining wall, by Kötter† and Boussinesq. As the latter has discussed local effects particularly, it is believed his results may be applied to the pile.‡

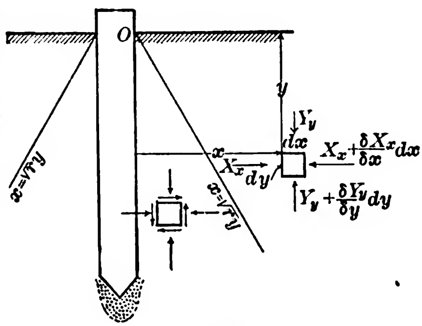

FIG. 5.

The equilibrium equations, being independent of z, or the direction in the length of the wall or piling, reduce to

$$\frac{\delta X_x}{\delta x}+\frac{\delta X_y}{\delta y}=0 \quad \text{for the } x\text{-direction,}$$

$$\frac{\delta Y_x}{\delta x}+\frac{\delta Y_y}{\delta y}+(\rho\, y=w)=0 \quad \text{for the } y\text{-direction.}$$

The region of perturbation is supposed to extend to the line the equation of which is

$$x=\sqrt{\frac{1-\sin.\phi}{1+\sin.\phi}}\;y=\sqrt{r}\;y,$$

where the coefficient $\sqrt{r}$, is the square root of the Rankine ratio. This must here be tentatively assumed. (See Fig. 5.)

* "Application des potentiels à l'étude de l'équilibre et du mouvement des solides élastiques," 1885 (Gauthier-Villars, Paris).

† "Erddrucken auf Stützmauern," Müller-Breslau (Stuttgart, 1906), pp. 107 *et seq.*

‡ *Annales des Ponts et Chaussées.* T. III, pp. 625-643.
See also "Theory of Elasticity," Todhunter-Pearson, Vol. II, Pt. II, p. 347; and *Minutes of Proceedings*, Inst. C. E., Vol. LXV, p. 212.

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The general Rankine relation is assumed to hold outside of this region at a distance from the pile, namely,

$$\sin.^2 \phi = \frac{(X_x - Y_y)^2 + 4\ X_y}{(X_x + Y_y)^2},$$

which states the expression for the "stability of a mass of earth in terms of the pressure at a point referred to any pair of rectangular axes, OX' and OY', in the plane of greatest and least pressure."* Taking $4\ X_y = 0$, the common expression is easily derived.

As a justification of the use of the above, it is assumed here that the plunger and cylinder experiments of Goodrich give a fair confirmation. (Whether $\frac{1}{5000}$ in. $\pm$ movement of the "plug" will permit the inference that the pressure on the plug is of the same intensity as that on the walls of the cylinder has always raised a query in the writer's mind.†)

Since the weight at the surface, assumed flat, is zero, the boundary relations become, by the vanishing of terms in the equations for the tetrahedron:

$$Y = 0, \qquad Y_x = X_y = 0.$$

At the sheet-piling (the retaining wall in the problem of Boussinesq), there results $X_y = \tan.\ \phi_1\ X_x$, where $\tan.\ \phi_1$ is the tangent of the angle of obliquity of the resultant pressure on the vertical face at the pile.

Boussinesq assumes "that in practice sustaining walls are generally sufficiently rough to render a thin stratum of the pulverulent mass stationary upon them. Hence the angle of friction between wall and mass really reduces to the angle of friction of the pulverulent mass upon itself."‡ This certainly is the maximum value. Kötter differentiates, however, the obliquity upon the x-face from that on the θ-face. In the case of sand, on account of dilatancy, the writer will follow the Boussinesq assumption, but for soils not granular or pulverulent will obtain such values for the later numerical computations as may be had from actual tests. Cain, Darwin, and others follow Boussinesq in retaining-wall design in this respect.

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* *Philosophical Transactions*, Royal Society, Vol. 147, p. 18.

† *Transactions*, Am. Soc. C. E., Vol. LIII, p. 283.

‡ "History of Elasticity," Todhunter-Pearson, Vol. II, Pt. II, p. 336.

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The following solution is given for the equations of equilibrium:

$$X_x = -\frac{1 - \sin. \phi}{1 + \sin. \phi} w\, y,$$

$$X_y = 0,$$

$$Y_y = - w\, y,$$

to apply without the region limited by $x = \sqrt{\frac{1 - \sin. \phi}{1 + \sin. \phi}}\, y$, and these are the ordinary Rankine relations. Within this region, or in the zone of perturbation of the pile, the following equations hold:

$$X_x = -\frac{1 - \sin. \phi}{1 + \sin. \phi} \frac{(y + x \tan. \phi)\, w}{1 + \sqrt{\frac{1 - \sin. \phi}{1 + \sin. \phi}} \tan. \phi}$$

$$Y_y = -\frac{(y + x \tan. \phi)\, w}{1 + \sqrt{\frac{1 - \sin. \phi}{1 + \sin. \phi}} \tan. \phi}$$

$$Y_x = X_y = \frac{\tan. \phi_1 \sqrt{\frac{1 - \sin. \phi}{1 + \sin. \phi}} \left(\sqrt{\frac{1 - \sin. \phi}{1 + \sin. \phi}}\, y - x\right) w}{1 + \sqrt{\frac{1 - \sin. \phi}{1 + \sin. \phi}} \tan. \phi}.$$

In the above set of constituents, the stresses, X_x and $Y_x = X_y$, are induced stresses, that is, they are called into play on a hypothetical infinitesimal motion outward of a retaining wall by the pressure head, Y_y. In the Rankine language, they stand to each other in the relation of "cause to effect." The pressure head of earth is "active," and the induced lateral stress is "passive."

In the case of piling, however, Y_x is the "active" stress. Accordingly, one would assume, very consistently, that the resultant stress on the x-face of a small element at the piling is active. To provide for this case, one might proceed in the ordinary manner of Rankine, namely, take $X \left(\frac{1 - \sin. \phi}{1 + \sin. \phi}\right)^2 \leqq Y_y$. This would appear to introduce ambiguities into the problem. The writer will proceed as follows:

Call the passive or smaller ratio of Rankine r_p and the active or larger ratio r_a. If Y_x and X_x are active, it seems reasonable to assume that the zone of perturbation due to pile action is larger. The wedge defining this region, the slant height of which is $x = \sqrt{r}\, y$, must intersect the head of the pile at the ground, because, whether

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"active" or "passive," the shears vanish at the pile for $y = 0$. Let $x = \frac{1}{\sqrt{r_p}} y = \sqrt{r_a}\, y$. The following is still true: At any point without the region the general Rankine relations hold. The constituents hold in general for all values of the variable within the region; the intensities become zero at the surface; while, at the pile, for $\phi = 0$, the ordinary Rankine relations still hold, the more general relations hold for ϕ_1.

To obtain a direct application of this, it is necessary to integrate the intensity, Y_x, over the surface of the pile at $x = 0$. First call

$$Y_{x0} = \tan.\ \phi_1 \frac{1 + \sin.\ \phi}{1 - \sin.\ \phi} \frac{w\, y}{1 + \sqrt{\frac{1 + \sin.\ \phi}{1 - \sin.\ \phi}} \tan.\ \phi_1} = \frac{f\, r_a}{1 + f \sqrt{r_a}}\, w\, y$$

for simplicity of expression, where f = the coefficient of friction at the pile, w = the specific weight of the earth, and y = the variable depth. To apply this intensity in practice, where cylindrical and slightly tapering piles are used, the assumption of Vierendeel and the others is made, that the tangential intensity is independent of the shape of the perimeter of the pile, a common enough assumption in other branches of engineering.

By integration there results for a working formula comparable with the Sanders' type in simplicity, but based on static considerations:

$$W = \frac{f\, r_a}{1 + f\sqrt{r_a}}\, w\, \pi\, D \int_o^L y\, dy,$$

$$\text{or } W = \frac{f\, r_a}{1 + f\sqrt{r_a}}\, w\, \pi\, D \frac{L^2}{2},$$

where πD is the mean circumference, L is the length of pile, w is the weight of a cubic unit of earth, f is the coefficient of friction, and r_a is the larger Rankine ratio, namely, $\frac{1 + \sin.\ \phi}{1 - \sin.\ \phi}$, ϕ being the angle of internal friction, or so-called angle of repose.

Now $w\, \pi\, D \frac{L^2}{2}$ is the normal hydrostatic pressure on a cylinder for w = specific weight. Accordingly, $\frac{f\, r_a}{1 + f\sqrt{r_a}}$ is a more or less rational friction factor for the same. While the formula is quite

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as simple as Vierendeel's, it would seem to possess a more rational derivation.

Effect of the Base.—In the above working formula, upward pressure on the base and sides, other than that due to tangential stresses, has been disregarded as relatively negligible. This will need to be discussed.

First, in the case of stiff earths possessing some elastic properties, where a more or less well-defined "conoid of pressure" may be assumed to exist, the pressure over the base of this conoid is naturally assumed to be continuous. The principle of equipollent loads (de St. Venant) shows that it is only in the region of the point that the real distribution of stress has any effect. In the case of a peg driven into a wooden beam and carrying a load on its head acting longitudinally to the axis of the peg, the local effect of the stress would be much the same whether the point of the peg entered a small knot-hole or butted against sound wood. The assumption, then, will be that the pressure under the pile is practically that which exists a foot or two horizontally away. In the horizontal projection of the lateral surface, the Y_y is assumed to be that for $x = 0$, $y = y_1$.

In the case of the Goodrich experiment, with the box and glass walls, when the model pile is pushed down in the sand close to the glass face the inverted paraboloid forming under the squared end of the pile is only two or three end diameters of the pile in height. The "eddy" action is largely confined to this small region. The Rankine hypothesis, of necessity, assumes that the action is felt at the surface, by reason of incompressible molecules arranged in most compact space. It is believed, however, as has been shown by Bauschinger, Darwin, and others, that considerable interstitial free space exists in any pulverulent soil; accordingly, when the pressure occurs it simply compacts the soil in the immediate region concerned. The assumption, then, for semi-liquid materials, it would seem to be reasonable, may be similar to that of the previous paragraph, namely, that the pressure at the point and sides suffers no sudden breaks or discontinuities from that a short distance away.

Accordingly, it is thought that the base and lateral buoyancy, when the point is down, may be amply provided for by taking L a few diameters longer, say to the point of the inverted paraboloid, instead of to the point of the pile, and using this length with the

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mean diameter of the pile. Such data, of course, would need to be determined experimentally; or, perhaps it might be better to consider the friction factor, $\frac{f\,r_a}{1 + f\sqrt{r_a}}$, simply as an empirical parameter to be determined for various cases.

Some Data.—In lieu of any precise coefficients of friction and angles of friction, no great precision can be expected in fitting the formula to actual cases. In the following the formula has been applied to the Annapolis* tests, J. P. Carlin, Assoc. M. Am. Soc. C. E., Engineer in Charge, also to the well-known Louisiana† pile (Proctorville, La., 1856-57).

TABLE 1.—ANNAPOLIS TESTS.

Number.	Length.	Point.	Butt.	Hammer.	Fall.	Actual test load.	Formula.	Goodrich $\frac{10\,W\,H}{8\,p}$	Remarks.
1	91	7	22	2 300	22	75 000	105 200	96 500	$L = \left\{ \begin{matrix} 60 \text{ ft. mud} \\ 6 \text{ ft. sand} \end{matrix} \right\} = 66$ ft.
2	91	7	22	"	22	85 090	133 610	112 000	$L = \left\{ \begin{matrix} 60 \text{ ft. mud} \\ 12 \text{ ft. sand} \end{matrix} \right\} = 72$ ft.
3	73	9	18	"	33½	34 000	95 450	67 000	L = 61 ft. of mud.
4	80	12	8	"	22	88 000	54 400	84 500	Sand.
5	82	13	9	"	22	110 000	66 500	168 700	Sand.

For Cases 1, 2, and 3, $f = 0.1$ and $\phi = 15°$ was used, for Cases 4 and 5, $f = 0.268 = \tan. \phi$, and $\phi = 15°$.

(See Patton's "Civil Engineering," 1st ed., p. 487, for actual test for f in liquid mud.)

w is taken at 110 lb. per cu. ft.

Case 1 is worked out below in full, to show the effect of vertical pressure on the side and base. The developed surface of contact is a trapezoid. The projected area on a horizontal plane is 1.45 sq. ft. The area of the base is 0.267 sq. ft.

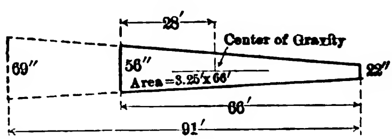

FIG. 6.

$$\frac{1 + \sin.\,15°}{1 - \sin.\,15°} = \frac{1.259}{0.741} = 1.70$$

$$\sqrt{1.70} \times 0.1 = 0.131$$

* *The Engineering Record*, May 11th, 1901; also *Transactions*, Am. Soc. C. E., Vol. XLVIII, pp. 215 and 218.

† Baker's "Masonry," 8th ed., p. 247.

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$$\frac{0.1 \times 1.70}{1.131} \times 110 \times 3.25 \times 66 \times 28 = \ \ 99\,500, \text{ friction on side,}$$

$$\frac{66}{1.131} \times 110 \times 0.267 = \ \ 1\,700, \text{ pressure on base,}$$

$$\frac{28}{1.131} \times 110 \times 1.46 = \ \ 4\,000, \text{ pressure on projected face,}$$

$$W = 105\,200, \text{ total calculated load.}$$

The common hydrostatic methods of area multiplied by mean head is used instead of the integration. The Rankine pressure on the base and projected side is 5 600 and 13 000 lb., respectively, for $\phi = 15$ degrees.*

For the Louisiana case, Baker's "Masonry" gives the following data: Pile was 12 in. square throughout, driven 29.5 ft., and bore 29.9 tons without settlement. It settled slowly under 31.2 tons. The same values of f and ϕ are used as in Cases 4 and 5 of the Annapolis test, namely, $f = 0.268$ and $\phi = 15°$, or

$$\frac{0.268 \times 1.70}{1 \times \sqrt{1.70 \times 0.268}} \times 110 \times 4 \times \frac{29.5^2}{2} = 64\,800, \text{ friction on sides,}$$

$$\frac{29.5}{1 \times \sqrt{1.70 \times 0.268}} \times 110 \times 1.0^2 = \ \ 2\,320, \text{ pressure on base,}$$

$$W = 67\,120, \text{ total calculated load.}$$

The static treatment presented gives an average deviation from fact about commensurate with that of the most rational dynamic formula. It is thought, however, that by obtaining actual experimental factors, based on the physical qualities of the pile, a much closer agreement would be possible. Most of the recorded data, being made solely with reference to their availability for comparison and study of dynamic formulas, omit such information.

The formula presented, being of the form of that given by Vierendeel, who neglects the basal action, it should be easy, by drawing tests, to acertain the friction, expressed as a function of length and mean diameter, for different soils.

Dilatancy of Granular Media.—In his interesting discussion of the Goodrich paper, the late Mr. Gould remarked:†

"Another element which makes for safety, but which baffles calculation, is the clinging action of the material through which the

* Note that 100 lb. instead of 110 lb. per cu. ft. will give about 10 000 lb. smaller.

† *Transactions*, Am. Soc. C. E., Vol. XLVIII, p. 214.

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pile is driven, and which action is set up immediately after it has been allowed to come to rest. It is often impossible to draw a defective pile even a very short time after it has been driven, unless a few blows be given by the hammer to start it, when it may come up very easily."

It is believed that the theory of the dilatancy of media composed of rigid particles in contact, as proposed by Professor Osborne Reynolds,* will account for this phenomenon noticed and recorded by many engineers. While the theory was formulated to account for the sub-mechanics of the universe, not the least of its claims is that it will place the theory of earth pressures on a true foundation. He says:

"I will point out the existence of a singular fundamental property of such granular media which is not possessed by known fluids or solids. * * * I have called this unique property of granular masses 'dilatancy,' because the property consists in a definite change of bulk, consequent on a definite change of shape or distortional strain, any disturbance whatever causing a change of volume and generally dilation.

"In the case of fluids, volume and shape are perfectly independent; and although in practice it is often difficult to alter the shape of any elastic body without altering its volume, yet the properties of dilation and distortion are essentially distinct, and are so considered in the theory of elasticity. In fact there are very few solid bodies which are to any extent dilatable at all.

"With granular media, the grains being sensibly hard, the case is, according to the results I have obtained, entirely different. So long as the grains are held in mutual equilibrium by stresses transmitted through the mass, every change of relative position of the grains is attended by a consequent change of volume; and if in any way the volume be fixed, then all change of shape is prevented."

The mathematics of this is long and difficult, in general. The essential features, as it is desired to apply them in reference to Mr. Gould's remarks, may be illustrated by the following experiment:

"If we have in a canvas bag any hard grains or balls, so long as the bag is not nearly full it will change its shape as it is moved about; but when the sack is approximately full a small change of shape causes it to become perfectly hard. There is perhaps nothing surprising in this, even apart from familiarity; because an inextensible sack has a rigid shape when extended to the full, any deformation diminishing its capacity, so that contents which did not fill the sack at its greatest extension fill it when deformed. On careful consideration, however, many curious questions present themselves.

* *Philosophical Magazine* (London, E., & D.), Vol. XX, 1885, pp. 469 *et seq.;* also Reynolds' Works, Vol. III.

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"If, instead of a canvas bag, we have an extremely flexible bag of india-rubber, this envelope, when filled with heavy spheres (No. 6 shot), imposes no sensible restraint on their distortion; standing on the table it takes nearly the form of a heap of shot. This is apparently accounted for by the fact that the capacity of the bag does not diminish as it is deformed. In this condition it really shows us less of the qualities of its granular contents than the canvas bag. But as it is impervious to fluid, it will enable me to measure exactly the volume of its contents.

"Filling up the interstices between the shot with water so that the bag is quite full of water and shot, no bubble of air in it, and carefully closing the mouth, I now find that the bag has become absolutely rigid in whatever form it happened to be when closed.

"It is clear that the envelope now imposes no distortional constraint on the shot within it, nor does the water. What then, converts the heap of loose shot into an absolutely rigid body? Clearly, the limit which is imposed on the volume by the pressure of the atmosphere.

"So long as the arrangement of the shot is such that there is enough water to fill the interstices the shot are free, but any arrangement which requires more room is absolutely prevented by the pressure of the atmosphere * * *.

"The very finest quartz sand, or glass balls ¾ in. in diameter, all give the same results."

It would seem that such a state of affairs would tend to exist after the driving, in the final rearrangement of the particles in granular soils, and that the phenomenon may throw light on the case, as cited by Mr. Gould. It would further seem to favor an elastic theory, especially as one, to use another of Professor Reynolds' illustrations, may note the firmness of a sandy beach after the recession of a wave, in contradistinction to the quite fluid effect of the dry sand. The phenomenon deserves to be studied in its relation to the pile.

The writer was led to appreciate the importance of the static point of view, in the theory of the pile, through the suggestions of G. S. Williams, M. Am. Soc. C. E. He was introduced to the Boussinesq and Kötter theories* by Professor Alexander Ziwet. In making acknowledgment to these authorities and to Professor A. B. Pierce for discussion and criticism of these theories, the writer does not wish to be construed as committing them to these views.

* A number of solutions involving different phases of this problem may be easily found. Among these the writer would call attention to a treatment of the glacier by Hopkins, Cambridge Phil. Soc. *Transactions*, Vol. 8, 1849, in which he treats the glacier as an elastic body forcing its way between the walls of the valley, exerting lateral forces and friction on the sides of the stream quite comparable to the action of a pile in the earth. The Boussinesq literature, however, affords the most suggestion in addition to that of Kötter.

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DISCUSSION.

Mr. Wagoner.

LUTHER WAGONER, M. AM. SOC. C. E. (by letter).—The writer believes this to be a timely paper, and that, with sufficient data concerning the physical properties of the resisting media, it will be possible to construct a satisfactory formula for computing the safe bearing load of a pile by purely statical methods. The assumption of uniform values of the angle, ϕ, is probably incorrect, and in what follows the writer will consider a soil composed of mud to an indefinite depth. The top layer of such a mud offers but little resistance to penetration, and the resistance appears to increase more rapidly than that of the static pressure due to depth. In tests made by the writer in Islais Creek, which is an arm of San Francisco Bay, the top mud is seen to be full of blow-holes, and relatively there is much water to each unit of solid. A sample of the material taken at a depth of 10 ft., or 15 ft. below mean tide, weighed 105 lb. per cu. ft. When dried at steam heat it lost 34% of its weight, or 35.7 lb. of water per cu. ft., which left 0.428 cu. ft. of volume for 69.3 lb. of solids, or $\frac{69.3}{0.428} = 162$ lb. per cu. ft. for the weight of the solid material, which is about the weight of the rock from which the mud is derived.

From the foregoing this important deduction can be made: The mud is the débris of rocks, in an exceedingly fine state of subdivision, mixed with some clay. Any unit of this material possesses an enormous surface as compared with its volume, and to this surface the water is held by capillary action. Both the water and the separate units of the mud are practically incompressible. The mixture is also incompressible, and as long as it is saturated it must continue to behave as an imperfect fluid. For example, a pile driven into such material does not compress the soil laterally to any extent, but elevates the soil near the pile. A blanket of rock or earth can be floated upon the mud, and there will be little or no subsidence if the lateral flow can be prevented by retaining walls, or, if a flat enough slope is given to the mud thus covered it will stand. A sharp distinction must be made for soils above and below the permanent ground-water level; in the first case, the soil has voids, once occupied by water, and is compressible by pile-drivers, and it is in such cases that a conical pile gives good results. In the second case, there can be no compression, but there is an upward displacement.

The mass of mud may be thought of as made up of small flattish particles of solids, arranged, as a rule, nearly level, and the interstitial space filled with water, or as a solid having an infinite number of capillary tubes of variable diameters. It is obvious that under suffi-

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Mr. Wagoner.

cient pressure the water will move laterally through the tubes, and the solids will be brought closer together, at the same time, the diameters of the tubes will be reduced, and a greater head or pressure will be required to produce further flow; but, as compression ensues, and the diameters of the tubes are reduced, the rate of subsidence must become slower; and, if for total subsidence we take y, and for time x, it is clear that the resulting curve must be an asymptote to the x axis.

That the above is sound reasoning is borne out by various known settlements, during long periods of time, where structures have been founded upon clay or muds. In San Francisco a notable example of settlement has occurred: A roughly semicircular body of land, one mile long and half a mile wide, was reclaimed from the bay, and upon this the business section of the city is built. A sea-wall, made by dumping rock into a dredged trench, marks the outer boundary. Inside of the sea-wall the streets have sunk slowly below grade, while adjacent buildings upon long piles have remained intact; a total subsidence of several feet, perhaps from 5 to 8 ft., has taken place in the past thirty years. The subsidence of the sea-wall has been very small in amount, certainly not greater than one-tenth of the landward subsidence. This appears to the writer to be a case of steady loading causing the water to percolate slowly seawards, thus allowing settlement to occur.

S. W. Hoag, Jr., M. Am. Soc. C. E., in testing the soil for the Chelsea Docks, New York City, where the mud is 180 ft. deep, drove four groups of four piles each to a penetration of 50 ft., loaded each group with concrete blocks, and noted the rate of subsidence. The experiment ended at 51 days, but the curves of subsidence show a total movement of from 1½ to 3 in. where the load was 18 tons on a plain pile and 34.6 tons on a lagged pile. The curves mentioned above are markedly asymptotic to the time axis. These experiments deserve careful study by any one desiring data regarding the behavior of piles in a mud soil.*

It should not be forgotten that a formula may give correctly the immediate or present ultimate bearing load of a pile, and yet serious damage may arise from slow and long-continued settlements; and it is the writer's belief that the attention of the investigator should be turned toward the physics of the soil; thus, probably in a rational formula, he will be able to forecast settlement as well as immediate loads.

Mr. Griffith.

John H. Griffith, Assoc. M. Am. Soc. C. E.—Since this paper was printed, the writer's attention has been called to the fact that, in the Annapolis tests, by Mr. Carlin, some of the piles had several

* Report to John A. Bensel, Engr. in Chief, N. Y. Docks, Nov. 24th, 1902.

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Mr. Griffith. feet of water above the top surface of the earth surrounding them. In a more general treatment, such a case should be considered, that is, the boundary relation at the surface should be satisfied for the head of water existing, instead of zero, as in the case worked out. If the different strata of soil are considered, this will complicate the problem still further.

At the time of this criticism, the writer made some approximate calculations modifying the result for Case 1 which would indicate that the water pressure would cause an increase in the value given for this pile of about 10 000 to 12 000 lb.

In a practical theory, it will be necessary to assume a homogeneous or isotropic soil medium, as has been done heretofore in engineering studies of earth pressure; also to adhere to the common assumption of an upper surface free from stress, as this will apply to the greater number of cases in experience. Otherwise, it will be necessary to increase the specific weight of the earth a proportionate amount, or actually modify the stress constituents. All the problems of engineering are really beyond the realms of analysis in the rigorous aspects, and it is only by defining the media specifically, or averaging the conditions, that practical solutions may be effected.

Another point has been raised, regarding the weight of the pile, or dead load, which did not appear in the calculations. Since it has been remarked to the writer that the specific weight of yellow pine or oak when soaked with water may even exceed that of water, so that such timbers will sometimes sink below the surface, the dead load will compensate the value given in the first criticism to the amount of several thousand pounds.

By far the most important factor in an elementary static analysis is the question of friction and internal angle of friction. The writer believes that in the main the experimental methods of approach heretofore in vogue are often more or less unsatisfactory because based on false premises of operation. Space will only suffice for one illustration in the pile theory, and that is the common notion advanced by many engineers that the friction in drawing a pile is equal to that encountered in driving it. This conclusion, if not absolutely erroneous, is believed to be not true in general. The argument can only be imperfectly stated, as follows: When the pile is driven the tubes of stress will start out from the periphery of the pile and spread over a correspondingly large area, probably in a conoidal distribution such as has been well described by Goodrich. Assuming that a cohesive material under pressure will be subject to the laws of elastic analysis, such a notion would appear to be confirmed by the well-known tests of Professors Carus-Wilson* on the beam for surface loading, and

* *Proceedings*, Physical Society of London, Vol. XI, 1891, p. 194. Also Sir G. G. Stokes' works. A good discussion is in Bovey's "Mechanics on Surface Loading of Beams."

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of Marston* for the roller problem, if the principle of equipollence is strained a little from its usual applications. On the other hand, in withdrawing the pile, such is not the case. One might indeed conceive of the conoid as inverted, with its base at the free surface of earth, in which case, for the pulverulent material, the friction on the periphery would be equated to the weight of the material under discussion. Mr. Griffith.

The treatment of Patton will illustrate this point in the fact that he uses equations for minimum and maximum loadings, respectively, taking $\frac{1 - \sin. \phi}{1 + \sin. \phi}$ for the minimum and $\frac{1 + \sin. \phi}{1 - \sin. \phi}$ for the maximum lateral factor, according to Rankine. Now, if the pile is being driven, the $\frac{1 + \sin. \phi}{1 - \sin. \phi}$ is operative while $\frac{1 - \sin. \phi}{1 + \sin. \phi}$, the reciprocal, holds if for any reason the pile tends to rise. Accordingly, assuming the coefficient of friction to be the same, as in the ordinary cases in other fields of engineering, the total friction on the pile in the case of withdrawal is less than that in driving. But, for a cohesive soil, it would naturally be expected that a closer agreement would hold between the two cases.

From the discussion by Mr. Gould on the paper by Mr. Goodrich, quoted previously, the writer would be led to infer that a considerable arching action of the ring elements around the pile, which are subject to hoop compression, exists; and, after the pile moves upward, it encounters little resistance, due to the earth friction, by reason of this; but, whether the friction at the start in withdrawal is equal to that in driving, opens a path for discussion by the profession. It is hoped that engineers who have the opportunity to withdraw piles may compare their results with the loading the pile previously carried, or was supposed to carry.

The whole static problem would appear to be a more general statement of the problem usually ascribed to Cerruti and amplified in its various phases by Boussinesq, Hertz, and various mathematicians, viz.:

When an elastic cylinder or cone of revolution, initially free from stress, is inserted in an elastic medium of relatively large extent, the upper surface of which is flat or nearly so, and normal to the axis, what are the strains and stresses in both due to vertical loading of the cylinder?

For the practical purposes of the engineer, he will specialize this problem to the case of a rigid pile and a pulverulent medium, and ignore the discontinuity at the base for a first approximation. Professor Burr, in his "Mechanics of Materials," has given a derivation of the equations of equilibrium in cylindrical co-ordinates which will be

* *Transactions*, Am. Soc. C. E., Vol. XXXII, pp. 99 and 273.

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Mr. Griffith. more easily understood by the practical engineer than by attempting the transformation of co-ordinates usually given in the treatises. The tubes of stress may be considered to start from the surface of the pile with an angle of inclination to the axis equal to the angle of friction of the earth on the pile. The principal stress is co-axial with this tube. It will travel downward and radially until ultimately the integrity of the tube is destroyed and displacement of the particles takes place at some distance from the pile, in conformity with the Rankine analysis.

The Rankine equations will satisfy the equations at a distance from the pile, but cannot be assumed to hold locally on account of the tangential or friction stresses which must be distributed into the earth around the pile. Hence a correction or parameter must be inserted in the Rankine stress values, and the resulting values inserted in the equations of equilibrium. The values of these corrections must be those which are compatible with the conditions at the ground and at the pile. The resulting constituents must hold for all values within the region affected by the tangential stresses. What is this region? The writer's experiments thus far have shown rather unsatisfactory results. In the first approximation it will be natural to take the region as limited by a cone, but this does not seem to give close agreement with fact. The matter deserves investigation by those interested in placing the pile on a rational basis, where it belongs.

The writer has followed Mr. Wagoner's discussion with interest. His citation of experiments for getting the specific weight, etc., of a soil in its ocean bed is quite pertinent at this time.

With regard to the friction angle, the writer is prepared to agree that the assumption of "uniform values" of this is not rigorous, as has already been intimated by various investigators, say, Darwin and Wilson; but, for the purposes of the practical engineer, it must be assumed as constant for any particular medium, for reasons of expediency. Such expressions for the variation of the coefficient of friction, as have been given heretofore, are too complicated to introduce into the theory in the early stages of its evolution, but must ultimately find a place in a rational analysis of earth pressure. Most engineers will be content if they can approximate to the true status of loading, with a probable error of, say, 15 or 20% ±, such as one might expect in good bridge design.

The discussion of time rate of strain variation, involving questions of subsidence, soil viscosity, etc., enters a comparatively virgin field of study and experiment. Such investigations, considered in connection with Slichter's studies, already cited, will undoubtedly receive an important place in a final analysis of the pile, from the static point of view.

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SUMMARY.

Mr. Griffith.

The practical points which are brought out in this paper are as follows:

a.—Attempts to get at the loading on a pile by a dynamic theory are indirection of effort. The direct method is static, and should carry out the work inaugurated by Patton, Vierendeel, Desmond, and others. Both methods will co-operate in fixing load limits, and will serve as check operations.

b.—Their methods, while not rational of form, give an efficient means of first approximation. Their value may be augmented in efficiency by abandoning the Rankine ratio altogether and replacing by a constant, which constant is to be selected on the basis of actual tests from ultimate loads for similar soils, or from drawing tests when properly interpreted. Using this constant with the "hydrostatic pressure" due to earth, a simple integration gives the real load value empirically when multiplied by the coefficient of friction; or, more directly, owing to the uncertainty of this coefficient, it may be included in the determination of the constant.

c.—The notion of using the full Rankine pressure on the base is probably in error, except for short piles and shallow foundations. The Vierendeel form of neglecting this in comparison with that on the periphery would seem to be more favorable to fact in a continuous medium. This does not apply to a discontinuity at the base such as would be implied by a rock foundation under the pile.

d.—The pile is susceptible to an elastic treatment to a considerable extent. Engineers should determine the moduli of soils, pulverulent, plastic, or other, just as in steel or wooden structures. The fact of average values of moduli may apply as properly as it does in concrete or wood.

e.—The Rankine theory should ultimately be displaced in favor of an elastic treatment, because, by reason of strain, viscosity, and cohesion, it can never fit the facts.

f.—The question of dilatancy should be studied in its relation to the pile.

g.—Those who are interested in the development of a correct theory of the pile should preserve the physical data for a static analysis, such as that of the pile periphery, its slope, and diameters, also the soil data and stratification. It has been the writer's experience, in attempting to correlate figures with facts, that most of this has been rejected in the dynamic analysis.

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AMERICAN SOCIETY OF CIVIL ENGINEERS
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TRANSACTIONS

Paper No. 1176

REINFORCED CONCRETE PIER CONSTRUCTION.

By Eugene Klapp, M. Am. Soc. C. E.

With Discussion by Messrs. William Arthur Payne, and Eugene Klapp.

A private yacht pier, built near Glen Cove, Long Island, has brought out a few points which may be of interest. It is an example of a small engineering structure, which, though of no great moment in itself, illustrates the adoption of means to an end that may be capable of very great extension.

The problem, as submitted to the writer, was to construct a yacht landing at East Island, on the exposed south shore of Long Island Sound, in connection with the construction at that point of an elaborate country residence. The slope of the beach at this point is very gradual, and it was specified that there should be a depth of at least 4 ft. of water at low tide. Soundings indicated that this necessitated a pier 300 ft. long. It was further specified that the pier should be to some extent in keeping with the scale of the place being created there, and that a wooden pile structure would not be acceptable. Besides these esthetic conditions, wooden piles were rejected because the teredo, in this part of the Sound, is very active. At the same time, the owner did not care to incur the expense of a masonry pier of the size involved. Also, it was desired to unload on the pier all material for the house and grounds during construction, and coal and other supplies thereafter, thus necessitating a pier wide enough to allow access for a cart and horse and to provide room for turning at the pier head.

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PLATE XXX.
TRANS. AM. SOC. CIV. ENGRS.
VOL. LXX, No. 1176.
KLAPP ON
REINFORCED CONCRETE PIER CONSTRUCTION.

YACHT PIER NEAR GLEN COVE, N. Y.

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Comparative designs and estimates were prepared for (*a*) a pier of ordinary construction, but with creosoted piles; (*b*) a concrete pier on concrete piles; and (*c*) for a series of concrete piers with wooden bridge connections. The latter plan was very much the best in appearance, and the calculated cost was less than that of the pier of concrete piles, and only slightly more than that of creosoted piles, the latter being only of a temporary nature in any case, as it has been found that the protection afforded by creosote against the teredo is not permanent.

At this point on the Sound the mean range of the tide is about 8 ft., and it was determined that at least 5 ft. above mean high water would be required to make the underside of the dock safe from wave action. There is a northeast exposure, with a long reach across the Sound, and the seas at times become quite heavy. These considerations, together with 4 ft. of water at low tide and from 2 to 3 ft. of toe-hold in the beach, required the outer caissons to be at least 20 ft. high.

To construct such piers in the ordinary manner behind coffer-dams, and in such an exposed location, was to involve expenditure far beyond that which the owner cared to incur. The writer's attention had shortly before been called to the successful use of reinforced concrete caissons on the Great Lakes for breakwater construction, by Major W. V. Judson, M. Am. Soc. C. E., and under patents held by that officer. It seemed that here was a solution of the problem. These caissons are constructed on the shore, preferably immediately adjoining the work. After thorough inspection and seasoning, they are usually launched in a manner somewhat similar to a boat, are towed into position, sunk in place, and then filled with rip-rap.

In this case what was needed was a structure that could be constructed safely and cheaply in the air, could then be allowed to harden thoroughly, and could finally be placed in accurate position. The weights to be supported were not great, the beach was good gravel and sand, fairly level, and, under favorable circumstances of good weather, the placing of the caissons promised to be a simple matter. Therefore, detailed plans were prepared for this structure.

An effort was made to preserve some element of the yachting idea in the design, and bow-string trusses, being merely enlarged gang planks, were used to connect the caissons.

The pier was originally laid out as a letter "L," with a main leg

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of 300 ft. and a short leg of 36 ft. The pier head consisted of eight caissons in close contact, and was intended to form a breakwater, in the angle of which, and protected from the wave action, was to be moored the float and boat landing. After the first bids were received, the owner wished to reduce the cost, and every other caisson in the pier head was omitted, so that, as built, the pier contains eight caissons and five 53-ft. trusses. The caissons supporting the trusses are 8 ft. wide and 12 ft. long, and those in the pier head are 12 by 12 ft. On account of the shoal water and the great height of the outer caissons in comparison with their cross-section, it seemed advisable to mould them in two sections. The reinforcement in the side walls consisted of round ½-in. rods horizontally, and ⅜-in. rods vertically, spaced as shown on Fig. 1, together with cross-diaphragms as indicated.

The caissons were reinforced for exterior pressures, which were to be expected during the launching and towing into position, and also for interior pressures, which were to be expected at low tide, when the water pressure would be nothing, but the filling of the caissons would be effective. The corners were reinforced and enlarged. In order to secure a proper bedding into the sand foundation, a 12-in. lip was allowed to project all around the caisson below the bottom. In the bottom there was cast a 3-in. hole, and this was closed by a plug while the lower section was being towed into place.

The question of the effect of sea water on the concrete was given much thought. The writer is unable to find any authoritative opinions on this subject which are not directly controverted by equally authoritative opinions of a diametrically opposite nature. He thinks it is a question that this Society might well undertake to investigate promptly and thoroughly. There can be no question that there are many distressing instances of failures due to the action of sea water and frost on concrete, and that many able and experienced engineers in charge of the engineering departments of the great transportation companies have simply crossed concrete off their list of available materials when it comes to marine construction. It is a subject too large in itself to be discussed as subsidiary to a minor structure like the one herein described, and though many have rejected concrete under these conditions, other engineers equally conservative are using it freely and without fear.

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The writer consulted with his partner and others at some length, and, considering all the advantages to accrue by the use of these concrete caissons, decided to do so after taking all known precautions.

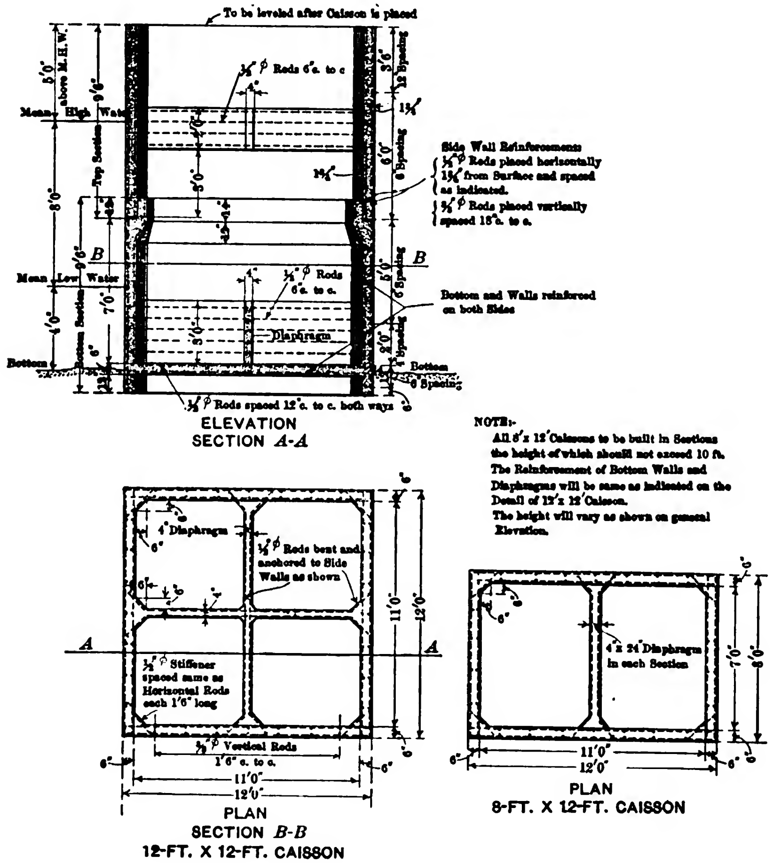

Fig. 1.

These precautions consisted in:

First, the use of cement in which the chemical constituents were limited as follows:

It was specified that the cement should not contain more than 1.75% of anhydrous sulphuric acid (SO_3) nor more than 3% of magnesia (MgO); also that no addition greater than 3% should have been made to the ingredients making up the cement subsequent to calcination.

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Secondly, to secure by careful inspection the most completely homogeneous mixture possible, with especial care in the density of the outer skin of the caissons.

Thirdly, a prolonged seasoning process before the new concrete should be immersed in the sea water.

In addition to these well-known precautions, it was decided to try the addition to the cement of a chemical element that should make with the free lime in the cement a more stable and indissoluble chemical combination than is offered by the ordinary form of Portland cement. This was furnished by the patent compound known as "Toxement," which is claimed by the inventor to be a resinate of calcium and silicate of alumina, which generates a resinate of lime and a silicate of alumina in crystalline form. It is further claimed that each of these materials is insoluble in sodium chloride and sodium sulphate, 3% solution. It was used in all the caissons, excepting Nos. 1 and 2, in the proportions of 2 lb. of Toxement to each 100 lb. of cement. The first two caissons were not thus treated, and will be held under close observation and comparison with the others, which were treated with this compound.

The mixture used was one of cement (Pennsylvania brand), two of sand, and four of gravel. The sand and gravel were from the nearby Cow Bay supply, and screened and washed. None of the gravel was larger than ½ in., grading down from that to very coarse sand. The sand was also run-of-bank, and very well graded.

The caissons, after being placed, were filled with sand and gravel from the adjoining beach up to about mean high-water mark, and the edges outside all around were protected from tidal and wave scour by rip-rap of "one man" stone.

The trusses were constructed on a radius of 34 ft., with 8 by 8-in. chords, 6 by 6-in. posts, and 1-in. rods. The loading was figured as a loaded coal cart plus 100 lb. per ft. All lumber was clear yellow pine, except the floor, which was clear white oak. The pipe rail and all bolts below the roadway level, and thus subject to frequent wettings by salt water, were of galvanized iron. The trusses were set 9 ft. 9 in. apart on centers, giving a clear opening of 8 ft. between the wheel guards under the hand-rails. The fender piles were creosoted. The float was 18 ft. long and 12 ft. wide.

A contract was let to the Snare and Triest Company, and work

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was commenced early in August, 1909. The first caisson was poured early in September, and the last about the beginning of October.

The caissons were all cast standing on parallel skids at about mean high water. It was first intended to construct a small marine railroad and launch the caissons in that manner, rolling them along the skids to the head of the marine railway. This plan was abandoned, however, and by sending in at high tide a powerful derrick scow, many of the caissons were lifted bodily from their position and set down in the water, towed to place and sunk in position, while the others, mostly the upper sections, were lifted to the deck of the scow and placed directly from there in their final position. There was not much difficulty in getting them to settle down to a proper bearing. Provision had been made for jetting, if necessary, but it was not used. In setting Caisson No. 2 a nest of boulders was encountered, and a diver was employed to clear away and level up the foundation. The spacing was accomplished by a float consisting of two 12 by 12-in. timbers, latticed apart, and of just sufficient length to cover the clear distance between the caissons. The first caissons being properly set inshore, the float was sent out, guyed back to the shore, and brought up against the outer edge of the set caisson. The next caisson was then towed out, set against the floating spacer, and sunk in position. There was some little trouble in plumbing the caissons, but, by excavating with an orange-peel bucket close to the high side and depositing the material against the low side, they were all readily brought to a sufficiently vertical and level position to be unnoticed by sighting along the edge from the shore.

The trusses were all constructed in the contractor's yard at Bridgeport, and were towed across the Sound on a scow. They were set up and braced temporarily by the derrick boat, and then the floor and deck were constructed in place.

On December 26th, 1909, a storm of unusual violence—unequaled in fact for many years—swept over the Sound from the northeast; the waves beat over the pier and broke loose some floor planks which had been only tacked in position, but otherwise did no damage, and did not shift the caissons in the least. The same storm partly destroyed a pier of substantial construction less than a mile from the one in question.

Unfortunately, the work was let so late in the summer, and the

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restrictions as to seasoning the concrete were enforced so rigidly, that the work of setting the caissons could not be commenced until November 11th, thus the entire construction was forced into the very bad weather of the late fall and early winter. As this involved very rough water and much snow and wind, the work was greatly delayed, and was not completed until the middle of January. The cost of the entire dock was about $14 000.

The writer believes that the cost was much less than for masonry piers by any other method of construction, under the existing circumstances of wind, tide, and exposure.

It would seem that for many highway bridges of short span, causeways, and similar structures, the use of similar caissons would prove economical and permanent, and that they might be used very largely to the exclusion of cribwork, which, after a decade or so, becomes a source of constant maintenance charges, besides never presenting an attractive appearance. Finally, in bridges requiring the most rigid foundations, these caissons might readily be used as substitutes for open wooden caissons, sunk on a prepared foundation of whatever nature, and still be capable of incorporation into the finished structure.

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DISCUSSION

WILLIAM ARTHUR PAYNE, M. AM. SOC. C. E. (by letter).—On the arrival of the first barge load of brick, to be used in building a residence on the estate to which this pier belongs, a severe northwest wind blew for two days, after the boat was moored alongside, directly against the head of the pier and the side of the boat. The effect on the pier was to crush the fender piles and cause a settlement of one of the caissons at the pier head on the west end. The caisson was knocked slightly out of alignment, and a settlement toward the west was observable. Mr. Payne.

The writer believes that this was caused by the pounding of the brick barge on the sand bottom on which the cassions rest, during half tide, the boat being raised from the bottom on a roller, and striking when the roller had passed. In order to protect the pier and avoid the bumping of barges against it, three groups of piles were driven about 8 ft. beyond the end, a secondary platform was built between these and the stringer of the pier, and arranged so that it would slide on the stringer in case of movement of the piles. This secondary platform is particularly advantageous in the handling of material, as the height of the dock was found to be excessive for passing up brick and cement. For handling material after it is deposited on the dock, an industrial railroad has been built. At the shore end of this railroad, brick and cement are dumped into wagons, in which they are carried up the hill to the house.

EUGENE KLAPP, M. AM. SOC. C. E. (by letter).—The injury done to the piers, as reported by Mr. Payne, is not to be wondered at. The pier was primarily built for a yacht landing, and, on account of the shoal water conditions, excepting at extreme high tide, it was mostly to be used by tenders and launches from larger yachts. It was thought that at high water the large steam yachts might be able to come alongside. Mr. Klapp.

Provision was not made for tying up to the dock a heavily loaded brick scow and allowing it to remain there through rough weather.

The building of the secondary fender piles, during the temporary use of the dock for unloading building material, will doubtless prevent further damage.

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AMERICAN SOCIETY OF CIVIL ENGINEERS

INSTITUTED 1852

TRANSACTIONS

Paper No. 1177

FINAL REPORT OF SPECIAL COMMITTEE ON RAIL SECTIONS.*

Your Special Committee on Steel Rails, since their appointment in 1902, have held numerous meetings, not only of their own body, but also in conference with Committees representing other Societies and the steel rail makers. The results of their deliberations have been presented to the Society in their reports presented on—

January	21st,	1903†
"	18th,	1905
"	17th,	1906
"	16th,	1907
July	9th,	1907
December	6th,	1907
"	18th,	1908
November	30th,	1909

As previously reported to you, the Rail Committee of the American Railway Engineering and Maintenance of Way Association is also acting for the American Railway Association; and the latter organization has guaranteed to it the necessary funds to make exhaustive tests and observations as to the wear, breakage, etc., etc., of steel rails. This work is being prosecuted, and will of necessity require several years.

Your Committee feels that it has nothing to add to the several reports which it has presented to the Society, particularly as, so far, the several cardinal principles outlined in them are being practically

* Presented to the Annual Convention, June 21st, 1910.

† These reports were published in *Proceedings*, Am. Soc. C. E., as follows: February, 1903, p. 48; February, 1905, p. 60; February, 1906, p. 50; February, 1907, p. 69; August, 1907, p. 290; February, 1908, p. 85; February, 1909, p. 61; February, 1910, p. 62.

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followed in the several used and proposed specifications and rail sections.

In view of the foregoing, your Committee would respectfully ask to be discharged so that the field may be clear if at any future time the Society should desire to again place the subject in the hands of a Committee.

JOSEPH T. RICHARDS,
C. W. BUCHHOLZ,
E. C. CARTER,
S. M. FELTON,
ROBERT W. HUNT,
JOHN D. ISAACS,
RICHARD MONTFORT,
H. G. PROUT,
PERCIVAL ROBERTS, JR.,
GEORGE E. THACKRAY,
EDMUND K. TURNER,

Approved in connection with the attached report:

WILLIAM R. WEBSTER.

JUNE, 1910.

PHILADELPHIA, JUNE 1ST, 1910.

I have signed the Report of the A. S. C. E. Rail Committee,—"Approved in connection with the attached report," as I feel that the report is too condensed, and assumes that all are familiar with the Rail situation, especially what has been done by the other Societies.

The work undertaken by this Committee has been delegated by The American Railway Association to the Rail Committee of The American Railway Engineering and Maintenance of Way Association, and it therefore seems appropriate to give the results of their work, up to date, to our members in convenient form for reference, especially as our rail specifications have not been worked to, and they have offered a better specification that will be worked to, and no doubt largely used by the members of this Society. The specification is attached to this report.

In presenting this specification to the Annual Meeting at Chicago in March last, the Committee said:*

"A new specification should not be proposed at this time without careful consideration. So far as we know, no railroad company has purchased rails under the specifications approved by the American Railway Association and referred to us; nor do we know of any railway company that has succeeded in buying rails during the past two years according to a specification entirely satisfactory to the railroad company. We believe that all of the specifications under which rails have been rolled have been compromises on the part of both parties, with the general result that neither party is entirely satisfied. Our

* *Bulletin No. 118*, December, 1909.

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experience during the year has brought to our attention some defects in all of the specifications now before us, and acting under the impression that there is a distinct feeling that we should revise our specifications, we offer the attached specifications for your consideration. Our Association has no specification for Open-Hearth Steel Rails, and in order to comply with the instructions, a specification for Open-Hearth Steel Rails is included.

"We believe it necessary to submit a sliding scale for the percentages of carbon and phosphorus, which provides for increasing the carbon as the phosphorus decreases. The fixing of this scale properly is a matter requiring care, and we admit that our knowledge on the subject is limited. The American Railway Association specification calls attention to this matter in the following words: 'When lower phosphorus can be secured, a proper proportionate increase in carbon should be made.' The amount of increase is not provided for in the specifications, and this appears to us to be necessary in order to secure uniformity of practice; otherwise, the fixing of these percentages becomes a matter of special arrangement. Bessemer rails are being furnished regularly with phosphorus under the maximum allowed, and where this is done, the carbon should be raised above the higher limit now fixed in our specifications, or a soft and poor wearing rail will result; yet this condition has not been fully guarded against in rails furnished under existing specifications. The lower and upper limits for carbon have heretofore been fixed with the intention that the mills furnish rails with a composition as near between the two limits as possible. The mills, however, in order to meet the prescribed drop tests with the least difficulty, keep both the carbon and manganese as nearly as possible to the lower limits, with the corresponding result that a generally poor-wearing rail is furnished.

"Some roads have prescribed the limits of deflection to be allowed under the drop test. With our present knowledge, we believe that we should fix a minimum deflection to eliminate brittle rails and to secure greater uniformity of product; also maximum deflection to eliminate soft rails. We are not able at the present time to fix these limits, but our ultimate object will be to determine and fix such limits for the specifications.

"With reference to the amount of discard, time of holding in ladle, size of nozzles, and other such details of manufacture or machinery, we are of the opinion that the physical and chemical tests required should be prescribed, and that we should see that the material submitted for acceptance meets the prescribed tests. We should not dictate to the manufacturers the amount of crop which shall be removed from the top of the ingot, as this should vary with the care and time consumed at the various mills. The railroads should not be asked to take anything but sound material in their rails. The mills can furnish such sound material if the proper care and sufficient time are taken in the making of the ingots. Information derived from the tests being made at the Watertown Arsenal shows definitely that sound rails cannot be made from unsound ingots, and that, therefore, the prime requisite in securing a sound rail is to first secure the sound ingot.

"We recommend that the present Specifications for Steel Rails be

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withdrawn from the Manual of Recommended Practice of the Association, as no longer representing the current state of the art.

"We submit herewith, as Appendix 'A,' a form for specifications. It will have to be amended from time to time as we receive further information on the subject."

The specifications referred to above were modified and presented at the Meeting in *Supplement to Bulletin No. 121*, of March, 1910, and in this final form are attached hereto.

These specifications do not represent the work of any one Society or the work of any one Committee, but are the result of all the work of the different Societies, as the members of all are so interwoven that whatever work is done in any one Society, or by the Committee of a Society, has very naturally and fortunately been carried into the others.

At the Chicago Meeting these specifications were accepted without a single change, and this is very unusual and shows how generally acceptable they were, as the members of all Rail Committees were present at the Meeting. The main points in this specification were discussed and agreed upon by the members of the Committee and the Rail Committee of the manufacturers who have co-operated with them in this work.

In the matter of Rail Sections, the Rail Committee of The American Railway Engineering and Maintenance of Way Association has not arrived at any definite conclusions. The new sections "A" and "B" of The American Railway Association have not given as good results as was expected of them, and the whole matter is yet under consideration. The Committee reported as follows:*

"The instructions of the American Railway Association require us to study the A. R. A. sections 'A' and 'B' in use and submit a single type for standard. Owing to the conditions existing in 1908, very little rail was laid, and practically none of the A. R. A. sections, in such manner as to give the needed information. This year, several roads have laid A. R. A. sections of rail, with a view of determining the relative merits of the respective sections. These rails have been in the track so short a time that we are not justified in drawing any conclusions as to which of the A. R. A. types, 'A' or 'B,' or if either, is better than the A. S. C. E. sections.

"*Bulletin No. 116,* issued October, 1909, gives the statistics for rail failures for six months from October 31, 1908, to April 30, 1909, as reported to the Committee. These statistics do show that the difference in section can be entirely annihilated by difference in chemical composition and by the treatment in furnace and mill.

"The results so far obtained from the heavy base A. R. A. sections are disappointing, as we have received some rail from the mills of the new section which was as bad as we did with the old A. S. C. E.

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* *Bulletin No. 118*, December, 1909.

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section, showing that the quality of the rail does not depend entirely upon the section.

"The tests to be inaugurated by the Committee, combined with the results of the tests at Watertown and the performance of the rail in the track, will give us valuable data to aid us in coming to a final conclusion."

A careful study of the results already obtained, on both Bessemer and open-hearth steel rails, indicates that the next necessary step will be the use of a much heavier rail, and I think the sooner this is admitted and trial lots of say 1 000 tons each of 110-lb., 120-lb. and 130-lb. rails rolled, of Bessemer and open-hearth steel, and put in service under the most severe conditions, the sooner we will get rid of the present difficulties with our rails.

WM. R. WEBSTER.

"SPECIFICATIONS FOR STEEL RAILS.*

Process of Manufacture.

"1. The entire process of manufacture shall be in accordance with the best current state of the art.

"(*a*) Ingots shall be kept in a vertical position until ready to be rolled, or until the metal in the interior has had time to solidify.

"(*b*) Bled ingots shall not be used.

Chemical Composition.

"2. The chemical composition of the steel from which the rails are rolled shall be within the following limits:

	BESSEMER.		OPEN-HEARTH.	
	70 lbs. and over, but under 85 lbs.	85 to 100 lbs. inclusive.	70 lbs. and over, but under 85 lbs.	85 to 100 lbs. inclusive.
Carbon	0.40 to 0.50	0.45 to 0.55	0.53 to 0.66	0.63 to 0.76
Manganese	0.80 to 1.10	0.80 to 1.10	0.70 to 1.00	0.70 to 1.00
Silicon	0.07 to 0.20	0.07 to 0.20	0.07 to 0.20	0.07 to 0.20
Phosphorus not to exceed	0.10	0.10	0.04	0.04
Sulphur not to exceed	0.075	0.075	0.06	0.06

"3. When the average phosphorus content of the ingot metal used in the Bessemer Process at any mill is below 0.08 and in the Open-Hearth Process is below 0.03, the carbon shall be increased at the rate of 0.035 for each 0.01 that the phosphorus content of the ingot metal used averages below 0.08 for Bessemer steel, or 0.03 for Open-Hearth steel.

"The percentage of carbon in an entire order of rails shall average as high as the mean percentage between the upper and lower limits.

Shearing.

"4. The end of the bloom formed from the top of the ingot shall be sheared until the entire face shows sound metal.

"All metal from the top of the ingot, whether made from the bloom or the rail, is the top discard.

*Reprinted from *Supplement to Bulletin No. 121* of the American Railway Engineering and Maintenance of Way Association (March, 1910).

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"5. The number of passes and speed of train shall be so regulated that, on leaving the rolls at the final pass, the temperature of the rails will not exceed that which requires a shrinkage allowance at the hot saws, for a 33-ft. rail of 100 lb. section, of 6½ in. for thick base sections and 6¾ in. for A. S. C. E. sections, and ⅛ in. less for each ten pounds decrease of section, these allowances to be decreased at the rate of 1-100 in. for each second of time elapsed between the rail leaving the finishing rolls and being sawed. **Shrinkage.**

"The bars shall not be held for the purpose of reducing their temperature, nor shall any artificial means of cooling them be used between the leading and finishing passes, nor after they leave the finishing pass.

"6. The section of rail shall conform as accurately as possible to the templet furnished by the Railroad Company. A variation in height of 1-64 in. less or 1-32 in. greater than the specified height, and 1-16 in. in width of flange, will be permitted; but no variations shall be allowed in the dimensions affecting the fit of splice bars. **Section.**

"7. The weight of the rail shall be maintained as nearly as possible, after complying with the preceding paragraph, to that specified in the contract. **Weight.**

"A variation of one-half of one per cent. from the calculated weight of section, as applied to an entire order, will be allowed.

"Rails will be accepted and paid for according to actual weight.

"8. The standard length of rail shall be 33 ft. **Length.**

"Ten per cent. of the entire order will be accepted in shorter lengths varying by 1 ft. from 32 ft. to 25 ft.

"A variation of ¼ in. from the specified lengths will be allowed.

"All No. 1 rails less than 33 ft. shall be painted green on both ends.

"9. Care shall be taken in hot-straightening rails, and it shall result in their being left in such condition that they will not vary throughout their entire length more than four (4) in. from a straight line in any direction for thick base sections, and 5 in. for A. S. C. E. sections when delivered to the cold-straightening presses. Those which vary beyond that amount, or have short kinks, shall be classed as second quality rails and be so marked. **Finishing.**

"The distance between supports of rails in the straightening press shall not be less than forty-two (42) in.; supports to have flat surfaces and out of wind. Rails shall be straight in line and surface and smooth on head when finished, final straightening being done while cold.

"They shall be sawed square at ends, variations to be not more than 1-32 in., and prior to shipment shall have the burr caused by the saw cutting removed and the ends made clean.

"10. Circular holes for joint bolts shall be drilled in accordance with specifications of the purchaser. They shall in every respect conform accurately to drawing and dimensions furnished and shall be free from burrs. **Drilling.**

"11. The name of the manufacturer, the weight of the rail, and the month and year of manufacture shall be rolled in raised letters and figures on the side of the web. The number of the heat and a letter indicating the portion of the ingot from which the rail was made shall **Branding.**

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be plainly stamped on the web of each rail, where it will not be covered by the splice bars. Rails to be lettered consecutively A, B, C, etc., the rail from the top of the ingot being A. In case of a top discard of twenty or more per cent. the letter A will be omitted. Open-Hearth rails to be branded or stamped O. H. All marking of rails shall be done so effectively that the marks may be read as long as the rails are in service.

Drop Testing. "12. (*a*) Drop tests shall be made on pieces of rail rolled from the top of the ingot, not less than four (4) ft. and not more than six (6) ft. long, from each heat of steel. These test pieces shall be cut from the rail bar next to either end of the top rail, as selected by the Inspector.

"The temperature of the test pieces shall be between forty (40) and one hundred (100) degrees Fahrenheit.

"The test pieces shall be placed head upward on solid supports, five (5) in. top radius, three (3) ft. between centers, and subjected to impact tests, the tup falling free from the following heights:

70 lb. rail	16 ft.
80, 85 and 90 lb. rail	18 ft.
100 lb. rail	20 ft.

"The test pieces which do not break under the first drop shall be nicked and tested to destruction.

"(*b*) (It is proposed to prescribe, under this paragraph, the requirements in regard to deflection, fixing maximum and minimum limits, as soon as proper deflection limits have been decided upon.)

Tests. "13. (A) Two pieces shall be tested from each heat of steel. If either of these test pieces breaks, a third piece shall be tested. If two of the test pieces break without showing physical defect, all rails of the heat will be rejected absolutely. If two of the test pieces do not break, all rails of the heat will be accepted as No. 1 or No. 2 classification (according as the deflection is less or more, respectively, than the prescribed limit*).

"(B) If, however, any test piece broken under test A shows physical defect, the top rail from each ingot of that heat shall be rejected.

"(C) Additional tests shall then be made of test pieces selected by the Inspector from the top end of any second rails of the same heat. If two out of three of these second test pieces break, the remainder of the rails of the heat will also be rejected. If two out of three of these second test pieces do not break, the remainder of the rails of the heat will be accepted, provided they conform to the other requirements of these specifications, as No. 1 or No. 2 classification (according as the deflection is less or more, respectively, than the prescribed limit*).

"(D) If any test piece, test A, does not break, but when nicked and tested to destruction shows interior defect, the top rails from each ingot of that heat shall be rejected.

Drop-Testing Machine. "14. The drop-testing machine shall be the standard of the American Railway Engineering and Maintenance of Way Association, and have a tup of 2 000 lbs. weight, the striking face of which shall have a radius of five (5) in.

* Note: The clause in brackets in Sections A and C to be added to the specifications when the deflection limits are specified.

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"The anvil block shall be adequately supported and shall weigh 20 000 lbs.

"The supports shall be a part of or firmly secured to the anvil.

"15. No. 1 rails shall be free from injurious defects and flaws of all kinds. **No. 1 Rails.**

"16. Rails which, by reason of surface imperfections, are not accepted as No. 1 rails, will be classed as No. 2 rails, but rails containing physical defects which impair their strength, shall be rejected. **No. 2 Rails.**

"No. 2 rails to the extent of five (5) per cent. of the whole order will be received. All rails accepted as No. 2 rails shall have the ends painted white, and shall have two prick punch marks on the side of the web near the heat number near the end of the rail, so placed as not to be covered by the splice bars.

"Rails improperly drilled, straightened, or from which the burrs have not been properly removed, shall be rejected, but may be accepted after being properly finished.

"Different classes of rails shall be kept separate in shipment.

"All rails shall be loaded in the presence of the inspector.

"17. (*a*) Inspectors representing the purchaser shall have free entry to the works of the Manufacturer at all times while the contract is being executed, and shall have all reasonable facilities afforded them by the Manufacturer to satisfy them that the rails have been made in accordance with the terms of the specifications. **Inspection.**

"(*b*) For Bessemer Steel the Manufacturer shall, before the rails are shipped, furnish the Inspector daily with carbon determinations for each heat, and two complete chemical analyses every twenty-four hours representing the average of the other elements specified in section 2 hereof contained in the steel, for each day and night turn respectively. These analyses shall be made on drillings taken from the ladle test ingot not less than ¼ in. beneath the surface.

"For Open-Hearth Steel, the makers shall furnish the Inspectors with a complete chemical analysis of the elements specified in section 2 hereof for each melt.

"(*c*) On request of the Inspector, the Manufacturer shall furnish drillings from the test ingot for check analysis.

"(*d*) All tests and inspections shall be made at the place of manufacture, prior to shipment, and shall be so conducted as not to unnecessarily interfere with the operation of the mill."

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AMERICAN SOCIETY OF CIVIL ENGINEERS

INSTITUTED 1852

TRANSACTIONS

Paper No. 1178

ADDRESS AT THE 42D ANNUAL CONVENTION, CHICAGO, ILLINOIS, JUNE 21ST, 1910.

BY JOHN A. BENSEL, PRESIDENT, AM. SOC. C. E.

I know that to some of my audience a satisfactory address at a summer convention would be like that which many people regard as a satisfactory sermon—something soothing and convincing, to the effect that you are not as other men are, but better. While I appreciate very fully, however, the honor of being able to address you, I am going to look trouble in the face in an effort to convince you that, in spite of great individual achievements, engineers are behind other professional men in professional spirit, and particularly in collective effort.

Whether this, if true, is due to our extreme youth as a profession, or our extreme age, is dependent upon the point of view; but I think it is a fact that will be admitted by all that engineers have not as yet done much for their profession, even if they have done considerable for the world at large.

Looking backward, our calling may properly be considered the oldest in the world. It is older, in fact, than history itself, for man did not begin to separate from the main part of animal creation, until he began to direct the sources of power in Nature for the benefit, if not always for the improvement, of his particular kind. In Bible history, we find early mention of the first builder of a pontoon. This creditable performance is especially noted, and the name of the party principally concerned prominently mentioned. The same thing cannot be said of the unsuccessful attempt at the building of the first sky-

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scraper, for here the architect, with unusual modesty, has not given history his name, this omission being possibly due to the fact that the building was unsuccessful. If an engineer was employed on this particular undertaking, the architect had, even at that early stage of his profession, learned the lesson of keeping all except his own end of the work in the background.

The distinctive naming of our profession does not seem, however, to go back any farther than the period of 1761, when that Father of the Profession, John Smeaton, first made use of the term, "engineer," and later, "civil engineer," applying it both to others and to himself, as descriptive of a certain class of men working along professional lines now existing and described by that term.

Remarkable progress has certainly been made in actual achievements since that time, and I know of nothing more impressive than to contemplate the tremendous changes that have been made in the material world by the achievements of engineers, particularly in the last hundred years. This was forcibly impressed upon me a short time ago, while in the company of the late Charles Haswell, then the oldest member of this Society, who, seeing one of the recently built men-of-war coming up the harbor, remarked that he had designed the first steamship for the United States Navy. The evolution of this intricate mass of mechanism, which, from the very beginning of its departure from the sailing type of vessel, has taken place entirely within the working period of one man's life, is as graphic a showing of engineering activity as I think can be found.

Our activities are forcibly shown in many other lines of invention and in the utilization of the forces of Nature, particularly in the development of this country. We, although young in years, have become the greatest railroad builders in history, and have put into use mechanical machines like the harvester, the sewing machine, the telephone, the wireless telegraph, and almost numberless applications of electricity. Ships have been built of late years greatly departing from those immediately preceding them, so that at the present time they might be compared to floating cities with nearly all a city's conveniences and comforts. We have done away with the former isolation of the largest city in the country, and have made it a part of the main land by the building of tunnels and bridges. In all our work it might be said that we are hastening, with feverish energy, from one problem

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to another, for the so-called purpose of saving time, or for the enjoyment of some new sensation; and we have also made possible the creation of that which might be deemed of doubtful benefit to the human race, that huge conglomerate, the modern city.

There has been no hesitancy in grappling with the problems of Nature by engineers, but they seem to be diffident and neglectful of human nature in their calculations, leaving it out of their equations, greatly to their own detriment and the world's loss. We can say that matters outside of the known are not our concern, and we can look with pride at our individual achievements, and of course, if this satisfies, there is nothing more to be said. But it is because I feel that engineers of to-day are not satisfied with their position, that I wonder whether we have either fulfilled our obligations to the community, or secured proper recognition from it; whether, in fact, the engineer can become the force that he should be, until he brings something into his equations besides frozen figures, however diverting an occupation this may be.

One may wonder whether this state of affairs is caused from a fear of injecting uncertain elements into our calculations, or whether it is our education or training which makes us conservative to the point of operating to our own disadvantage. We may read the requirements of our membership and learn from them that in our accomplishments we are not to be measured as skilled artisans, but the fact remains that, to a great extent, society at large does so rate us, and it would seem that we must ourselves be responsible for this state of affairs. Our colleges and technical schools are partly to blame for the existence of this idea, on account of the different degrees which they give. We have a degree of civil engineer, regarded in its narrowest sense, of mining engineer, mechanical engineer, electrical engineer, and by necessity it would seem as if we should shortly add some particular title to designate the engineer who flies. In reality there should be but two classes of engineers, and the distinction should be drawn only between civil engineers and military engineers. As a matter of fact, fate and inclination determine the specialty that a man takes up after his preliminary training, and so far as the degrees are concerned, the only one that has any right to carry weight, because it is a measure of accomplishment, is that which is granted by this Society to its corporate members. The schools, in their general mix-up of titles,

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certainly befog the public mind. It is as if the medical schools, for instance, should issue degrees at graduation for brain doctors, stomach doctors, eye and ear doctors, etc. Very wisely, it seems to me, the medical profession and the legal profession, with histories far older than ours, and with as wide variations in practice as we have, leave the variations in name to the individual taste of the practitioner, in a manner which we would do well to copy. The Society itself has adopted very broad lines in admission to membership, classing as civil engineers all who are properly such; and there is good reason for the serious consideration of the term at this time, as we cannot fail to recognize a tendency in State and other governments to legislate as to the right to practice engineering. It was owing to the introduction of a bill limiting and prescribing the right to practice in the State of New York, that a committee was recently appointed to look into this matter and report to the Society. This report will be before you for action at this meeting.

As to the manner in which engineers individually perform their work, no criticism would properly lie, and in fact it is fortunate that our work speaks for itself, for, as a body, we say nothing. We are no longer, however, found working for the greater part of the time on the outskirts of civilization, and it becomes necessary, therefore, for us to change with changing conditions, and to use our Society not only for the benefit of the profession as a whole, but for the benefit of the members individually. Whether one of our first steps in this direction should be along legislative lines is for you to determine. For myself, having been confronted with legislation recently attempted in New York, I am convinced that we shall have legislation affecting our members, and this legislation should properly be moulded by some responsible body like our own Society. If we do not take the matter up ourselves it is likely to be taken up by other associations, and from past experience, it would seem as though it might be carried on along lines that would tend to ridicule our desire for professional standing.

The Society is to be congratulated on its present satisfactory status. The reports show a very satisfactory financial condition, and you may note a continuing increase in membership that is extremely gratifying. This, after having nearly doubled in the last seven years, still shows no sign of diminishing in its rate of increase. It may be

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said, also, that we have in the Society an excellent publishing house, where the members have an opportunity to secure technical papers published in the highest style of the art. We have in general in the officers, a number of men, who, within the prescribed limits, labor for the benefit of the members, but we also have constitutional limitations to the activity of our governing body, so that the voice of the Society is never heard, or, at least, might be compared to that still, small voice we call "conscience," which is not audible outside of the body that possesses it.

Now, in these days, when the statement that two and two make four is accepted from its latest originator as a newly discovered truth, a little extension of our mathematics, to take into our estimate people as well as things, is what we principally need, and it would be a good thing, regarded either from the point of view of what the world needs or the more selfish view of our own particular gains. At the present time it would seem as though our world had thrown away the old gods without taking hold of any new ones. Private ownership as it formerly existed is no longer recognized; individual action in almost any large field is to-day hampered and curtailed in a manner undreamed of twenty years ago. In fact, our whole scheme of government seems to be passing from the representative form on which it was founded, to some new form as yet undetermined. Whether all this is, in our opinion, for good or for evil, is of no particular concern. The matter that concerns us is, that we have left our old moorings, and that, to secure new ones, new limits are to be set to the activities of men along lines which concern us, and that, therefore, it is necessary that those who by education and training are best fitted to consider facts and not desires, should guide society as much as possible along its new lines. I consider that we as a profession are particularly trained to do this by our consideration of facts as they exist, and I think it will be recognized by all that we are not in our work or activities bound by any precedent, even if we do learn all that we can from the past; and that we are by nature and training of a cool and calculating disposition, which is surely a thing that is needed in this time of many suggested experiments.

To be effective, however, we must be cohesive, and thus be able to take our part not as the led, but as leaders, convincing the people, if possible, that all the ills of our social system cannot be cured by remedies which neglect the forces of creation, and that the best doctors for

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our troubles are not necessarily those whose sympathies are most audibly expressed.

In the recent discoveries of science our ideas as to the forces of Nature must be greatly enlarged and our theories amplified. Recent discovery of radium and radio-active substances shows at least that much of our old knowledge needs re-writing along the lines of our greater knowledge of to-day.

With this increase of knowledge it would seem as though those who devote their lives to the exploitation of natural forces should take a position in the future even more prominent than in the past, and it will undoubtedly become our function to help the world to that ideal state described by our greatest living poet of action, when he speaks of the time to come, as follows:

"And no one shall work for money,
  And no one shall work for fame;
But each for the joy of working,
  And each in his separate star;
Shall draw the thing as he sees it,
  For the God of the things as they are."

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MEMOIRS OF DECEASED MEMBERS.

LINUS WEED BROWN, M. Am. Soc. C. E.*

DIED MARCH 7TH, 1910.

In the death of Linus Weed Brown, which occurred in Monrovia, Cal., on March 7th, 1910, this Society lost one of its valued members and the Engineering Profession a most able exponent.

Mr. Brown was born in Burnside, Orange County, N. Y., in August, 1856, and received his early education in the schools of that town. He studied his profession in the Stevens Institute of Technology, Hoboken, N. J.

At the age of eighteen he entered the machine shops of the Pennsylvania Railroad, and later was employed as Draftsman by that Company, which position he held until 1880.

In 1880 he accepted a position with the Southern Pacific Railroad in New Orleans, La., and designed and supervised the construction of the Algiers shops.

In 1883 Mr. Brown severed his connection with the Southern Pacific Company and engaged in general engineering practice, principally in the line of sugar-house installations.

In 1885 he was elected Assistant City Engineer of New Orleans, which position he held for four years.

In 1890 he became Chief Engineer for the Caffrey Central Sugar Refinery, designing and supervising the erection of the buildings, which represented an expenditure of about $600 000. In the same year Mr. Brown was appointed Chief Engineer of the Franklin and Abbeville Railroad and built that road. At the same time he designed and built the Des Lignes sugar-house. In fact, he designed and built many of the large sugar mills and refineries erected in Louisiana about that time.

From 1892 to 1896 Mr. Brown held the office of City Engineer of New Orleans, and it was during this term that some of the most important works of his career were accomplished.

Under the direction of the City Council, and in consultation with B. M. Harrod, Past-President, Am. Soc. C. E., the late H. B. Richardson, M. Am. Soc. C. E., and Rudolph Hering, M. Am. Soc. C. E., Mr. Brown made a topographical survey of New Orleans, a study of precipitation and run-off, and prepared plans and specifications for a drainage system.

At the expiration of his term of office as City Engineer, he engaged in private practice, assuming charge, as Chief Engineer for the contractors, of the first construction work of the drainage system.

* Memoir prepared by Ole K. Olsen, Esq.

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Prior to and during his term of office as City Engineer, Mr. Brown was Architect of the McDonogh School Fund in New Orleans, during which time he designed and built several new schools and remodeled a number of old buildings.

He was also Special Engineer for the New Orleans Levee Board on harbor and bank protection work. To the study of this work Mr. Brown devoted all his energies and knowledge for several years. At the same time he was a member of the New Orleans Advisory Board of Engineers on Sewerage and Water.

When the oil fields of Texas were first discovered, Mr. Brown's services were immediately engaged, and in the following years he devoted almost his entire time to the development of the oil fields and facilities for handling the oil. His operations were principally in the Beaumont and Sour Lake fields.

The holdings of the Southern Pacific Railroad Company in these regions demanded the services of an expert engineer, and Mr. Brown was engaged to take full charge of its interests.

In 1904 he was compelled to give up active business and seek the restoration of his health. To this end he spent some time in the Middle Western States and finally decided to go out to the Pacific Coast. The climate there proved so beneficial that he eventually settled in Bakersfield, Cal., where he accepted an appointment as Consulting Engineer for the Oil Department of the Southern Pacific Railroad and Chief Engineer of the Atlantic Division of the same line.

Shortly after he accepted this appointment the Colorado River broke through its banks and overflowed the valley known as the Salton Sea, across which the tracks of the Southern Pacific Road were laid. The Company was compelled to make a detour of approximately 100 miles around the inundated region, but, under the direction of Mr. Brown, they succeeded in closing the break with two massive dams, confining the river to its ordinary channel and preventing the increase of the Salton Sea.

While in California Mr. Brown invented an oil and sand separator, which the Southern Pacific Company is now using throughout its oil fields. He also invented a continuous water purifier and a special oil power-pump. All these machines are now on the market.

Mr. Brown was a man of sterling integrity; one who regarded his profession in the light of an obligatory public service. To this sense of duty he sacrificed much, primarily the necessary relaxation and rest from arduous labor, which undoubtedly accelerated the end of his useful and honorable career.

In recognition of the valuable services he rendered in connection with the levee protection work in New Orleans, Mr. Brown was made the recipient of public honors and testimonials of appreciation.

He is survived by a daughter and two sons; the latter are preparing

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to follow the engineering profession. His wife, who was Miss Joan Von Vesterfeldt of New York City, died in 1903.

Mr. Brown was elected a Member of the American Society of Civil Engineers on June 7th, 1899. He was also a Member of the Louisiana Engineering Society.

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CHARLES ALFRED HASBROUCK, M. Am. Soc. C. E.*

DIED FEBRUARY 1ST, 1910.

Charles Alfred Hasbrouck was born at Forest Home, a suburb of Ithaca, N. Y., on July 31st, 1864. After studying in the schools at Ithaca, he entered Cornell University in 1880, from which, after completing a course in Civil Engineering, he was graduated in 1884, the youngest member of his class.

In July, 1884, Mr. Hasbrouck entered upon his professional career as Assistant Engineer of the Detroit Bridge and Iron Company, continuing with that firm until 1888. From August to November, 1888, he was employed with the King Bridge Company as Assistant Engineer.

In November, 1888, he was appointed Assistant Chief Engineer of the American Bridge Works, of Chicago, specializing in bridge and structural engineering.

In May, 1900, Mr. Hasbrouck was made Contracting Manager of the American Bridge Company, of New York, in charge of railroad structures on the Western Division, which position he held until his health failed. Thus, after 24 years of active service in his Profession, he was obliged to give up all work.

On June 14th, 1893, Mr. Hasbrouck was married to Miss Mary Fobes, of Cresco, Iowa, who died in 1907.

After retiring from business, Mr. Hasbrouck spent part of his time in El Paso, Tex., in search of health. In 1909, he went to Sierra Madre, and, later, to Pasadena, Cal., where he died on February 1st, 1910. He was a patient sufferer, never uttering a word of annoyance or fretfulness at his condition.

At his expressed wish, he was buried from his boyhood home which he had always kept up, and which, with its beautiful grounds, he left to Cornell University.

Mr. Hasbrouck was elected an Associate Member of the American Society of Civil Engineers on February 3d, 1892, and a Member on December 5th, 1894. He was elected a Member of the Institution of Civil Engineers, of Great Britain, on February 2d, 1904.

* Memoir prepared by Mr. Edward Capouch, Contracting Manager, American Bridge Company, Chicago, Ill.

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JOHN HENDERSON SAMPLE, M. Am. Soc. C. E.*

DIED MARCH 4TH, 1910.

John Henderson Sample, the only son of Judge William Sample, was born on April 3d, 1849, at Coshocton, Ohio. He entered Dennison University, Granville, Ohio, from which he was graduated in 1872. After leaving college, he was engaged on the early surveys of the Toledo and Ohio Central lines, working up from Axeman to Division Engineer.

Afterward Mr. Sample served as Chief Engineer of the Cincinnati, Lebanon, and Northern Railway, and Chief Engineer of the Cincinnati and Georgia (now the Southern Railway), from Rome to Macon, Ga., except from Austell to Atlanta. In 1883, he made surveys for the East Tennessee, Virginia and Georgia Railway (now the Southern Railway) in Alabama. He then became Chief Engineer of the Alabama Improvement Company, engaged in the location and construction of the Northern Alabama Railroad, and the development of coal and ore lands and the Town of Sheffield, Ala.

He was appointed Chief Engineer of the Toledo and Ann Arbor, on location and construction from Hammond Junction to Durand; Chief Engineer of location and construction of the Missouri Pacific lines in Kansas, Colorado, and Missouri; and from 1887 to 1889, he served as Chief Engineer on the construction of the Louisville, Henderson, and St. Louis Railway, from West Point to Henderson, Ky.

Mr. Sample made examinations and reports on timber and mineral lands in Kentucky, Tennessee, Virginia, and West Virginia, and in 1889, he examined and reported on the Mexican National Railroad, from Laredo, Tex., to the City of Mexico.

From 1889 to 1896, he was Chief Engineer of location and construction and General Superintendent of operation of the Pittsburg, Akron, and Western Railroad, from Delphos to Akron, Ohio. In 1897 he was appointed General Superintendent of the Cleveland, Akron, and Columbus Railroad, which position he held until this road was purchased by the Pennsylvania Company, in September, 1899. From that date to the time of his death, Mr. Sample was in the employ of the Pennsylvania Company, as Assistant Engineer, being engaged on line and grade revision and special work.

His father being a lawyer and Judge, he partook of his judicial nature, and all his lifework was based on the broad foundation of equity and honesty of purpose. He was a man of unobtrusive manner, retiring disposition, and unpretentious ways.

On June 7th, 1876, Mr. Sample was married to Miss Virginia Hughes. His wife died on June 24th, 1889.

* Memoir prepared by W. B. Hanlon, Esq.

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Mr. Sample died suddenly in the Fort Pitt Hotel, at Pittsburg, Pa., on March 4th, 1910. He intended to leave for New York City during the day to bid farewell to his son, who was Assistant Engineer on the Madeira and Mamoré Railway, in Brazil, and had been spending his vacation of three months with his father.

To his children, and to those who knew him intimately, Mr. Sample leaves a memory of a life well rounded out by noble endeavor, and a fixedness of purpose to know and do the right. He was conscientious in every act and thought, a man of deep religious conviction, and though called suddenly from his earthly labors, he was ready for the higher service and duty.

Mr. Sample was elected a Member of the American Society of Civil Engineers on October 6th, 1886.

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ALBERT MATHER SMITH, M. Am. Soc. C. E.*

Died February 27th, 1910.

Albert Mather Smith was born on October 5th, 1837, in New York City. He was the son of Charles Smith and Miss Alleta Loverich, and a direct descendant of Cotton Mather.

As a boy of fifteen he entered the Engineer Division of the Manhattan Gas Light Company, and later became Engineer of its West 18th Street Station. At the outbreak of the Civil War, Mr. Smith joined the 37th Regiment, New York Volunteers, organized by Colonel Roome, the President of the Manhattan Gas Light Company, and was chosen Captain of Company B. This Company was largely recruited from the force of the gas-works, and drilled in the office of the Gas Company at 4 Irving Place, New York City. Mr. Smith's regiment saw active service during the invasion of Pennsylvania, and also as special detail on the Chesapeake; and, later, during the Draft Riots in New York City.

After the close of the War, Mr. Smith became Chief Engineer of the Manhattan Gas Light Company, and, later, when this Company was merged into the Consolidated Gas Company, he became Engineer of Distribution of the latter Company. At the time of his death he had been connected with the gas companies of New York City for 57 years.

On March 18th, 1863, Mr. Smith was married to Miss Anna Provoost Elwes, who died on January 14th, 1873. In 1878, Mr. Smith was married to his second wife, Miss Jane H. Bull. His widow, two sons, and a daughter survive him.

Mr. Smith was a Charter Member and Vice-President of the Society of Gas Lighting, the oldest existing gas association in the United States. He was elected a Member of the American Society of Civil Engineers on May 5th, 1886.

* Memoir prepared by W. Cullen Morris, M. Am. Soc. C. E.

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JACOBUS VAN DER HOEK, M. Am. Soc. C. E.*

DIED DECEMBER 22D, 1909.

Jacobus Van der Hoek, son of the late Gysbertus Van der Hoek and Johanna (Tupers) Van der Hoek, was born at Goes, The Netherlands, on March 19th, 1862. He received his early education at the Public Schools, and was graduated from the High School of his native town in August, 1879. In September of the same year he entered the Polytechnic School at Delft, The Netherlands, from which he was graduated, as Civil Engineer, in July, 1883.

During 1884 Mr. Van der Hoek was employed as Inspector on the construction of a dike across the "het slaak," a shallow tidewater 1½ miles wide, and made surveys and soundings for a record map of adjacent waters covering an area of 6 sq. miles.

In 1885 and 1886 he was employed by the Dutch Government as Assistant Engineer in charge of a party, to re-survey the principal rivers of Holland, and triangulated about 25 miles of river.

During 1887 Mr. Van der Hoek was Engineer in charge of the submarine shore protection for the "Polder of Schouwen," The Netherlands. In 1887 he left his native land for the United States, arriving in New York City, on December 25th.

From the latter part of 1888 to the beginning of 1890, he was employed by the Wheeling Bridge and Terminal Railway Company, at Wheeling, W. Va., under the late Job Abbott, M. Am. Soc. C. E., Chief Engineer. The work comprised steam railway construction, a bridge 2 000 ft. in length, including one span over the Ohio River, 525 ft. long, and three tunnels from 400 to 2 400 ft. long, all double-track and heavy work throughout. The Engineer who was in charge of the work, writes:

"Mr. Van der Hoek reported to me as Chief Draftsman and Office Assistant during the period above mentioned. He was so capable and earnest in all of his work, and so well qualified to perform it, that our relations were not only uniformly pleasant, but they marked the beginning of a friendship that lasted until the deplorable end of Mr. Van der Hoek's useful life."

In 1890, Mr. Van der Hoek entered the service of the Lehigh Valley Railroad and continued with this Company until July, 1909; during this time he was successively engaged as Chief Draftsman, Assistant, Resident, and Division Engineer. During the extension of the main line of the Lehigh Valley Railroad, from Sayre to Buffalo, he was employed as Chief Draftsman, designing masonry and other structures, also as Assistant and Resident Engineer in charge of certain sections of the line. Paul S. King, M. Am. Soc. C. E., the Chief Engineer in

* Memoir prepared by F. E. Schall, D. C. Henny, H. F. Dunham and Paul S. King, Members, Am. Soc. C. E.

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charge of the construction of this 175 miles of double-track railroad, soon recognized the exceptional engineering ability of Mr. Van der Hoek, and appointed him, successively, Assistant and Resident Engineer in charge of several sections; of his success and ability, Mr. King writes:

"The sad and sudden death of Mr. Van der Hoek was indeed a great shock to me and his many friends in the Lehigh Valley System, particularly in New York State, his field of professional work for so many years.

"I highly regarded his technical ability, sterling character, and untiring industry, both in the field and office. During the time he was engaged with me (nearly four years), he filled the positions of Chief Draftsman, Assistant, and Resident Engineer, and earned the respective promotions by the zeal and energy which was always characteristic of him with any work he had in hand. He continued throughout the period of construction, a record not equalled by any of the dozen or more Resident Engineers connected with that work. It was this observation of his conduct and activity in executing his work that warranted me to have confidence in his ability to take up the work to be done after the Operating Department took charge of the line, recommending him as the Engineer for Maintenance of Way of part of the new line."

In 1893, Mr. Van der Hoek was appointed Division Engineer of the Buffalo Division of the Lehigh Valley Railroad, and had charge, under the Superintendent of Maintenance of Way, of constructing stations, water stations, coal trestles, wharves, stone ballasting the line, building storage yards, rebuilding bridges, etc.; he continued in this position until July 1st, 1909.

One of his associates on the Lehigh Valley Railroad writes:

"I was intimately acquainted with Mr. Van der Hoek and his work from 1894 to the time of his death, and as a co-worker on the Lehigh Valley Railroad it is a privilege to testify to his exceptional engineering ability, his strong, unflinching character, his untiring energy, and implicit adherence to the lines of duty. He had exceptional executive ability combined with a thorough knowledge of details. It was these qualities that made him so successful in his work.

"Mr. Van der Hoek was a sober, unassuming, and honest man, a generous and respected superior to his subordinates, a true friend, ever ready to assist an aspiring young man to greater knowledge and better positions; by these he will be truly missed and mourned."

On July 12th, 1909, Mr. Van der Hoek entered the service of the Lehigh Coal and Navigation Company, as Civil Engineer, under the General Superintendent of that company, at Lansford, Pa., to take charge of the railroad maintenance, water supply, land surveys, and new outside construction, on the extensive mining properties of that company in the anthracite coal fields.

Mr. Van der Hoek's exceptional ability was thoroughly recognized

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by his new employers, and his work and its results were fully appreciated; he had but laid his plans and perfected a proper organization when, on the afternoon of December 22d, 1909, while inspecting the work of laying a new water main through the Lansford, Pa., tunnel, he met his death by being run over by an engine, and his successful professional career was thus sadly ended. His Assistant, who had accompanied him on this inspection, met with the same lamentable fate.

On May 30th, 1896, Mr. Van der Hoek was married, in New York City, to Johanna Van der Bent, and is survived by his wife and two children.

He was elected a Member of the American Society of Civil Engineers on April 7th, 1897

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LUTHER ELMAN JOHNSON, Jun. Am. Soc. C. E.*

Died March 23d, 1910.

By the death of Luther Elman Johnson, the Engineering Profession has lost a bright and able young engineer whose career, though short, gave promise of a steady rise and a brilliant future.

Mr. Johnson, the son of Mr. and Mrs. M. D. Johnson, of Lawton, Okla., was born in Union, West Va., on August 10th, 1881. Most of his childhood and early manhood, however, were spent in Missouri. He received his High School training at Nevada, Mo., and his technical education at the Missouri State University, from which he was graduated in 1904, on his completion of the four years' course in Civil Engineering. In connection with the training at the University, Mr. Johnson, on graduation. was appointed and commissioned Brevet Second Lieutenant, in the National Guard of Missouri, by the Governor of the State.

His professional work began shortly after graduation, with his employment in the United States Reclamation Service, in connection with investigations of reservoir sites for the storage of irrigation water in Oklahoma. Following this, Mr. Johnson was transferred to the Garden City, Kans., pumping project, where, from 1905 to 1907, he was engaged in concrete construction and other work. In the latter part of 1907, he was transferred to the Minidoka, Idaho, pumping project, where, as Assistant Engineer, he was engaged until shortly before his death.

His work on the latter project was in connection with the location and construction of canals, and he was in active charge of the building of a large number of small reinforced concrete and timber structures and bridges for the irrigation system. In prosecuting this work, Mr. Johnson showed ability of the first order, and gave evidence, by his conscientious, thorough, and careful work, of great promise for the future.

In March, 1910, his health failing, he returned to his home in Lawton, Okla., to recuperate from a general breakdown, but pneumonia set in, and he died on March 23d.

Mr. Johnson was a young man of sterling qualities and rugged honesty; his life was clean and strong, his character sweet and lovable, and his capabilities exceptional. Untiring devotion to and interest in his work were traits which had won for him the deepest respect of his associates and those who worked under his direction, and his death was a keen loss, not only to his family to whom he was a devoted son and brother, but to his many friends and to all those with whom his work brought him in contact.

Mr. Johnson was elected a Junior of the American Society of Civil Engineers on September 6th, 1904.

* Memoir prepared by P. M. Fogg, Assoc. M. Am. Soc. C. E.

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